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WELCOMING ADDRESS

By

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It is a great honor for me to participate in arranging a scientific event of this magnitude. On behalf of the Office of Naval Research, the National Research Council, the Marine Biological Laboratory, the Woods Hole Oceanographic Institution, and the Fish and Wildlife Service, I take pleasure in welcoming every one of you to this oceanographic convocation.

To those of you who have come from abroad, I extend especial greetings and the wish that your trip to the United States will have proven most pleasurable in every way, as well as professionally rewarding.

Speaking as a naval officer, I cannot help but be delighted at the prospect of a three-day gathering devoted exclusively to the exchange of information on the study of the sea. The Navy is by far the largest consumer of oceanographic knowledge in the United States. The papers which are presented here and the ideas which are exchanged in your discussions are certain to prove valuable to the scientists and engineers working on naval problems.

The domestic economies of our respective countries also stand to benefit from these meetings. Some of you come from maritime nations, to which exploitation of the sea has meant far more than it has to a large continental country like the United States. As our population increases and our industry expands, the growing demands

upon the natural resources of this country will cause Americans also to turn more and more to the sea for resources. Development of the unexploited food resources of the oceans could mean an eventual higher standard of living for all peoples of the world.

I realize that this is not the fundamental purpose of your discussions. You are here as scholars, to contribute to the advancement of your profession. Your agenda, with its emphasis upon basic principles, is an impressive one.

We are all aware, however, that consequences develop rapidly from new ideas. Modern science exists in a highly conductive atmosphere in which ideas soon produce effects in the worlds of commerce and industry, politics, and military affairs. There is no doubt that the material progress which has marked man's history—and especially his recent history—would soon grind to a halt without a steady flow of that fresh knowledge which comes only from fundamental research.

I have mentioned such possible ramifications of your discussions only as speculation on the practical application of a conference that is devoted to the increase of knowledge. In arranging this convocation, the intent of my organization and of the other sponsors has been to encourage a mixing of ideas—that free exchange of information that is the lifeblood of scientific research.

We are honored by your attendance and certain that you will have a successful and an enjoyable meeting. If there is anything which we can do to make this convocation more attractive or to assist any of you in any way while you are here, please call upon the committee on arrangements.

RESEARCH ON THE COMPLEXITY OF H_2O

By

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It is truly a privilege and an honor to be here on behalf of the Naval Research Advisory Committee and to help in some small way in encouraging those of the expanded group who will work in the new building. It is my understanding that a talk at this time should be in the nature of an appetizer, since there is plenty of meat in the program proper.

Admiral Smith has told you that the Naval Research Advisory Committee is a statutory committee set up by Congress. To the best of my knowledge, the advisory committee to the Atomic Energy Commission is the only other scientific advisory committee of that distinction. You might ask, "What does such a committee do? How does it function?"

To start with, I would point out that it is supposed to consist of people who have had wide experience and who are recognized for their achievements and wisdom. For example, in the past the NRAC has comprised men such as Warren Weaver, Lee DuBridge, Louis DeFlorez, Lewis Strauss, and J. Robert Oppenheimer. Obviously this sounds like an important part of any roster of great living American scientists. Today the Committee comprises men of no less distinction, witness Drs. Rabi, Kelly, Stratton, Hunsaker, and Hutcheson, to mention just a few.

Obviously people of this type, having other responsibilities, cannot devote large numbers of days or weeks to detailed study of Navy projects. Their function is primarily advisory, to help set policy, and this they have done most effectively.

One of the more important aspects of policy is the maintenance of balance between development and research. As all of you know, research is one of those loose terms not well defined. Everyone has a different idea of research. To one man it is getting numbers from dynamometers. To another it is working out the solution of a differential equation. To still another it is synthesis for a new material of construction. There is no sharp line between research and develop-

ment, but human nature puts emphasis, effort, and money on those things that look immediate. Consequently, unless there is a conscious check and balance, development will always get an undue proportion of the effort and funds. The NRAC has realized the dangers involved in such a situation and has strongly expressed itself, with a resultant positive action by the Secretaries to maintain the basic research program.

The NRAC has also helped to open great bottlenecks. Presumably there is a much quoted law in the State of Kansas to the effect that, when two trains meet at a crossing, each train shall stop and neither shall proceed until the other has crossed the crossing. Unfortunately, we find a parallel in a great many situations, including some in Government research and development. One of the functions of the NRAC is to flag ahead one of the trains and unblock the situation.

For example, just before the National Science Foundation was first formed, the Office of Naval Research had increased war-time support of research in our universities in broad areas. When the Science Foundation was formed it might have seemed feasible for the ONR to drop support of all basic research except that which pertained directly to naval matters and let the Science Foundation carry on. However, the Science Foundation was a beautiful train on a track with no steam and no fuel for the boilers. At that time Congress had not appropriated much fuel. Rather than let the research stop, on advice of the NRAC the support of university basic work was continued by the ONR in areas that were perhaps somewhat less directly connected with the interests of the Navy. Incidentally, it is very difficult to ascertain just what field of basic research is not connected with naval warfare.

And now to my subject. I trust that the title has not deceived any of you. Obviously, if you talk about the complexities of H_2O, and research thereon, one first thinks of ionic bonds, covalent bonds, large molecules, surface tension perhaps, fluidity, viscosity under various conditions, vapor pressure, and the like. As I said, this talk is supposed to be an appetizer and not the meat. Any narrow conception of the problem of the complexity of H_2O, such as I have just indicated, could not possibly be covered in a half-hour talk. You must have much, much more time to get into this sort of detail. In a short talk such as this, it is necessary to have a broad subject so that the talk can be confined to generalities. Of course, sometimes it isn't confined to generalities. It may even include platitudes. I will try to avoid these.

We heard last night that some 75% of the surface of our planet is covered with water, H_2O. Physical research, in general, deals with

the relation of mankind to his environment and seeks knowledge of that environment for the benefit of mankind. Yet only a small percentage of the research that has been carried on in the last 20 decades has dealt with that 75% of our environment. So it is fitting that research on that environment be stimulated.

It is also true, as stated last night, that military demands and the impetus of war have been a terrific stimulant in scientific advances and invention. Psychologically it seems that if you have to do something to save your life, you do it, and quickly. If you have to do something just to live a little better, or if you do it for the sake of knowledge itself, time does not press and things move more slowly. But it is also true that, while many advances have resulted from military necessity or requirement, the great net benefit has been to our daily peace-time living. That is true of some things immediately connected with war, for example, dynamite. Actually, more dynamite is used now than was ever used in any war, and it is used for peaceful purposes.

Although the objective seems to be weapons of destruction and advances for the purpose of destruction, when we add it all up we find that it is the passing phase and that the real result and the real advance has made for better living.

I am sure you remember VanLoon's Geography of The World. He pointed out that on this planet the amount of space occupied by people is really quite small. The census states that there are about two billion people in the world; an easy calculation would show that the whole lot of them, theoretically at least, can be stacked in a cube a half mile on edge. This leaves a lot of "lebensraum" for anybody not in the box. If you don't like three dimensions, you can use two dimensions. If you want to put all of them inside a fence, you can have a circular enclosure with a 20 mile radius, in which case each one would have a little more than ten square feet. That, of course, is not much lebensraum. But it is not much of the planet's area either.

However, man uses quite a bit of space for food production. Malthus is the fellow who started this type of thinking. He had it all figured out that the human race couldn't possibly go on increasing for very many years because there wouldn't be enough space to grow enough food. Since then, by virtue of scientific advances, we have moved the decimal point on Malthus' calculations over so far that he begins to look a bit silly. On the other hand, there is a limit.

Even with a population of two billion we must go on, because it seems to be inherent in the mores of all cultures that the species must

propagate and must increase in order to have satisfaction. So we go on, and the population grows.

But how about the limit? The limit may or may not be food, as Malthus has predicted. We may run into other limitations. But consider food for the minute. Calculations have been made on food productivity from all of the land areas. As Admiral Smith has just pointed out, the sea is a major and wonderful potential source of food. I am not thinking now of the normal ways of winning foods from the sea, namely, catching fish, but rather what we might do, perhaps decades hence, with sea vegetation and the like.

In this connection one never knows how research in one area may provide a breakthrough in another.

The jet engine is rather recent. I doubt that many people have realized that the jet engine was made possible by research on the air compressor—on the turbine also—but primarily the compressor. And it is the improvement in the efficiency and effectiveness of the air compressor that has made the engine what it is today.

By the same token, pumps have not changed in character for many, many a year, decades, eons perhaps. You may say, "Well, the centrifugal pump, the rotary pump, is something new." Yes, it is relatively new. But just as research has resulted in a complete change in the mode of pumping air, so may we expect some development for moving water. I don't know what it is. If I did I would be hustling down to the Patent Office instead of standing here. But we may expect something in the way of developing pumps and moving water which would change the whole approach to handling the contents of the sea. When, is another story. We may even be smart enough to let that great pump, the moon, do the job for us. That pump, with its two strokes a day, really pumps a lot of water. Maybe we can do something about it.

There is a filtration problem, a precipitation problem, etc., but none of this scares anyone in industrial chemistry today. The energy to move the water and the modes of so doing are the things of concern. Once having done this, we have at our disposal a vast farm and a vast mine.

True, we are already taking magnesium out of sea water, also bromine, but pumping sea water for the salt isn't worthwhile. It is better to take more concentrated brines from below the surface of the earth only because it is too difficult to pump the water; it takes too much energy. Some day we will have atomic energy, economically. When it is economical enough perhaps we can use that to help pump water. Thus from atomic energy we will be getting food in an indirect fashion.

To conclude, we have a relatively untouched 75% of our environment on which to draw in order to improve our standard of living in the future. This we should realize. We should start and continue the basic research so that we can take advantage of the situation when the time comes.

THE ENCHANTED VOYAGE: A STUDY OF THE EFFECTS OF THE OCEAN ON SOME ASPECTS OF HUMAN CULTURE

By

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The symbolic and allegorical aspects of the sea and of the ship have been treated so fully and recently by Mr. W. H. Auden in his illuminating study *The Enchaféd Flood*,[1] that it would be presumptuous to attempt to say anything new about them from a literary point of view. Mr. Auden has considered his themes morally, religiously and tragically; what he has left to explore may at first seem frivolous or at best merely decorative, but when the exploration has yielded its results, some hint of a serious purpose may be apparent.

As a beginning, an examination of the first scene of *The Tempest* will be appropriate, since it introduces us to a world where terrestrial action and maritime action are in conflict. The situation reminds us of one of those extraordinary paintings of Magnasco in which a seascape seems to be overpowering a landscape.

The Tempest opens with the members of the royal household of Naples, who have authority on land, abusing the boatswain, who has authority on the ship. However, the old councillor Gonzalo, who is better and wiser than the other landsmen, expresses his confidence in the boatswain with these very remarkable words. "I have great comfort from this fellow: me thinks he hath no drowning mark upon him, his complexion is perfect gallows. Stand forth good Fate to his hanging: make the rope of his destiny our cable . . ." It is clear from this that the predicament in which the passengers found themselves would not end fatally.

Mr. T. S. Eliot, in *The Wasteland*, his first major work, used, as is well known, a number of images and phrases from *The Tempest*, Shakespeare's last work. In a certain sense *The Wasteland* is the converse or reciprocal of *The Tempest*. Of the images that Mr. Eliot rejected, at least initially, the hanged man is pre-eminent. Madame Sesostris, the fortune-teller with a somewhat unorthodox pack of tarot cards, remarks, "I do not see the hanged man. Fear

[1] W. H. Auden, *The Enchaféd Flood* or *The Romantic Iconography of the Sea*. Random House, New York, 1950.

death by water." In the fully developed lore of the tarot cards, the hanged man is given the meaning of the death of self as a prerequisite for illumination. Little enough is known of the origin of the tarot; it is not unreasonable to ally the hanged man to the Crucifixion, but this can hardly be a primitive meaning. Whatever the original interpretation may have been, Eliot consciously rejected the symbol, substituting for it the drowned man, Phloebas the Phoenician sailor, though his notes imply that the hanged man appears late in the poem in a shadowy form. For Eliot, the magical storm, which is innocuous in *The Tempest*, causes a disaster, and although all is not lost, the king is left fishing in a squalid world with an arid plane at his back and considering how he may salvage the little that is left of his possessions.

It is my purpose to show that an antithesis like that between the hanged man and the drowned man of *The Tempest* and *The Wasteland* has behind it much that is magical in the sea.

We may regard any oceanographic expedition as one sort of voyage, having some of the general properties of other voyages. There are a number of men in a ship. Traditionally, they are separated from women; they could not form a biologically stable community. Like any of the oceangoing sea-birds, man finds that he must return to the land to breed. Thus there is always the danger that the men on a voyage may be stranded on Circe's domain, or encounter the Sirens, or run aground on any of the numerous islands of women that are so persistent a feature of mythology.[2] Just as the modern expedition is in search of knowledge, so its mythological prototype is in search of magic. The voyage is undertaken to make the sea yield enchantment, which, though dangerous, may also be useful and beautiful.

All objects of the search will be things that still might be within the domain of any expedition studying biological oceanography. There will be time to consider only a few coelenterates and molluscs, precious coral, cowries, the sea snails that yield Tyrian purple, and pearls. One might add many other species, and one should not be surprised at the presence in the collection of quite unlikely organisms. For instance, there was probably an *Octopus* cult in the Mediterranean in prehistoric times, and this seems to have spread as far northwest as the island of Alderney.[3] It will not be possible, however, for one investigator to examine the whole collection, hence much of interest must be put on one side. Before beginning our study, it is desirable

[2] Anthony Thorne's *Young Man on a Dolphin* provides a recent and very delightful example.

[3] Kendrick (*The Archaeology of the Channel Islands*, Vol. I, *The Bailiwich of Guernsey*, London, 1928) figures a problematic stone object from a megalithic site on Alderney with the tentative suggestion that it is a cephalopod. There is no doubt that it represents *Octopus* or *Eledone*.

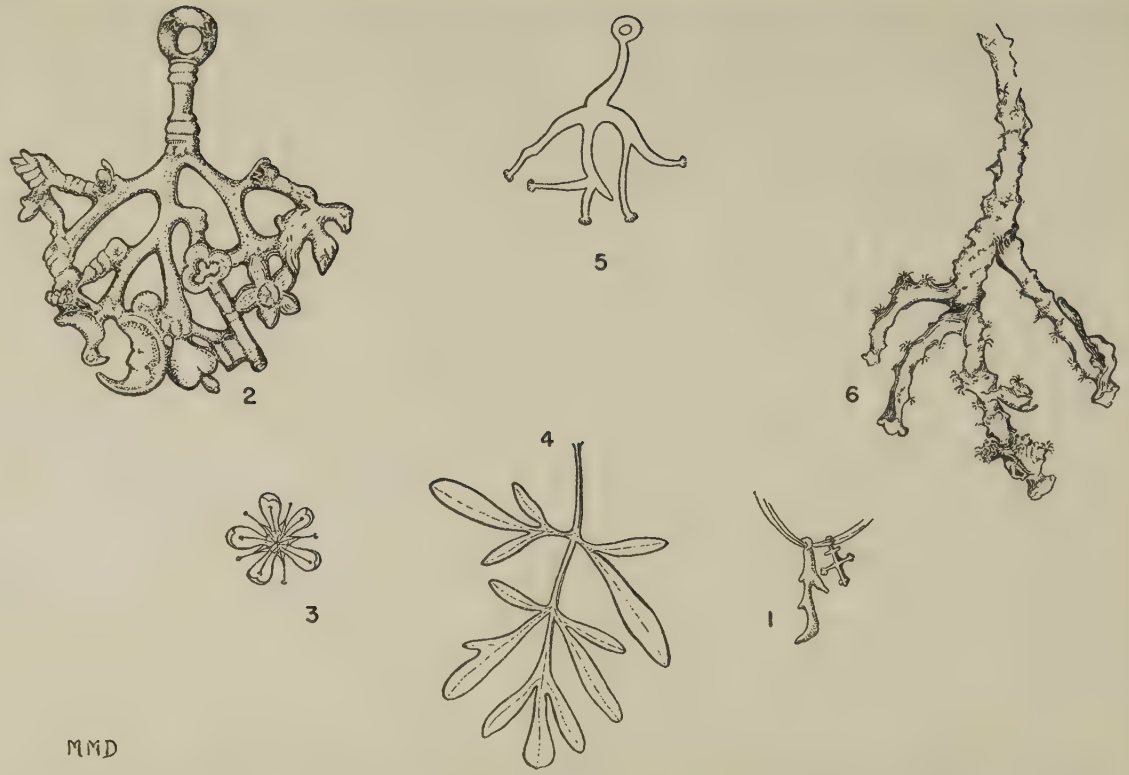
to remember that there is a persistant tendency to people the sea with beings paralleling those on land. When Alexander the Great is depicted in a mediaeval miniature of what Dr. Beebe has himself recognized as a prototype of the bathysphere, he is shown in a submarine landscape containing not only fishes but a thicket of submarine trees and a pair of submarine human beings, a merman and a mermaid. Today we have sea-horses, sea-cows, sea dogs (*canes marini* or dogfish, of which more hereafter), sea robins, sea urchins, sea anemones, as well as a few parallels of human artifacts such as sea pens and sea squirts. It will be clear, in fact, that the drowned man is in part a sea hanged man. For the moment, however, it may merely be noted that belief in such correspondences, which, incidentally, is mentioned in a more recondite form in the prefatory note to Shelley's *Sensitive Plant*, was very widespread.

The first specimens collected on our Enchanted Voyage belong to *Corallium rubrum*, the precious coral. On many early Italian paintings of the Madonna and Child, the latter wears at his throat a small branch of coral. In a few cases this is accompanied by some other ornament which may even be a cross (Fig. 1). The coral in some cases is a natural branch, somewhat polished; in other cases it may be worked into the shape of a horn. Such coral horns are still worn widely in southern Italy, where it is believed that they protect the wearer against the evil eye. Since children are considered to be particularly sensitive to such a baleful influence, it is reasonable that they should wear this ornament. In pagan times every Roman baby had hung about its neck, for the same purpose, a frankly phallic charm called the *res turpicula*, which was sometimes, though not invariably, made of coral.

There is also known, mainly in southern Italy,[4] a very remarkable charm which is used by pregnant women and which is hung at the necks of very small children. This charm is termed the *cimaruta* (Fig. 2) or sprig of rue. It is composite; in fact, some of the elements could be obtained separately. The standard components are a crescent moon, usually two birds, a key, and a flower which is supposed to be the rue (*Ruta graveolens*) flower (Fig. 3), though it lacks the stamens that alternate with the five petals.[5] An essential part of the charm is evidently one or more clenched hands, with the thumbs projecting between the index and middle fingers. This obviously

[4] I have been shown an example which is said to come from Florence. The great collection of these objects, that of Elworthy, is in the museum at Taunton, Somerset, England. See also F. T. Elworthy (*The Evil Eye*, London, 1895), from whose work some of my information is derived. However, Elworthy did not realize the connection between coral and *cimaruta*.

[5] Many rue flowers are in fact quadriradiate rather than quinqueradiate.



Figures 1–6. **1.** Coral and cross at the neck of the Christ child, from a painting by Giacomo di Mino del Pellicciaio (? 1342) in the Church of SS. Martino e Vittorio, Sarteano, Italy. **2.** *Cimaruta*, Naples (*coll.* Margaret Hutchinson). **3.** Flower of Rue **4.** Leaf of Rue. **5.** Bronze amulet, early Iron Age (Bologna Museum; after Elworthy). **6.** Branch of precious coral.

phallic gesture, protective against *malocchio* and known as *mano a fico*, is also found on the Roman *res turpicula*. In all good old examples, dating from the time of the Kingdom of the Two Sicilies, the ring at the top and sometimes other parts of the charm bear the hallmark NAP; it was considered important that the *cimaruta* be made of silver. There is little doubt that the moon, key, and rue flower are connected with Artemis or Hecate; the requirement that the charm be of silver fits a moon goddess also. However, when we extract these elements, and the birds, and *mano a fico*, we are left with a branching structure which is not much like a rue leaf (Fig. 4) and still less like a sprig of the plant; but it is almost identical with an early Iron Age object in the Bologna Museum (Fig. 5) which I believe is merely a bronze model of a branch of coral. Pliny tells us that coral was useful medicinally in infancy and that it also prevented nosebleeding. It may be significant that, within the past half century, pregnant women who were unable to afford a silver charm might stuff actual rue sprigs up their nostrils.[6] It would seem reasonably clear that the coral and the rue are to some extent interchangeable in prophylactic magic; from this point of view coral is a kind of sea-rue, or perhaps rue a kind of terrestrial coral.

As ordinarily understood, Artemis has no connection with the sea. She is a goddess of the moon, of the chase, a protector of virginity, a guardian of the nether world (as Hecate with her key), and in one form a goddess of marshes. The frog, her symbol in this last context, was widely used as a charm, at least until the late 19th century, if not more recently. A frog, however, is essentially a freshwater animal. The association of Artemis with coral is therefore not easily explained and can hardly be primary.

Another very common Neapolitan charm is in the form of a mermaid, often with two tails, or of a woman riding on a sea horse. Such a charm was called *Sirene*, so we now have caught another mythological being out of the sea. Though the purpose was the same as that of the *cimaruta*, the *sirene* was hung in a house or formed part of the elaborate ornaments of the harness of horses.[7] One remarkable feature of the *sirene* is that it was usually ornamented with bells. As Elworthy points out, similar bells formed part of an object called "coral and bells," which was customarily given to teething infants in England, at least down to the middle of the last century. It con-

[6] This information was given the writer in 1926 in Naples by Miss Florence Allen.

[7] Traditionally this was most elaborate; the writer has seen the central part swathed in black during Holy Week. Elworthy describes some of the objects involved, but a peculiar and not very common figure, that of a boy holding his shirt up to his waist in front and wearing a conical cap, looked very ancient and unexplained.

sisted[8] of a branch of coral with a rattle bearing the bells at one end. The coral and bells seems to link the belled siren with the coral horn and the silver *cimaruta*. That the charms are complex and inconsistent probably indicates a long and elaborate history. The association with Artemis or the sirens may well be a late aspect of that history. All that we can rightly conclude is that they involve some mythological lady out of the sea.

Of all such mythological ladies, none is more justly celebrated than Aphrodite, and when we turn to her we find a most curious situation. Hesiod, in his *Theogony*, says she arose from the sea foam. The ancient Greek philologists indeed derived her name from ἀφρός, but they were incorrect according to modern researches. She is usually supposed to be of eastern origin; the Greeks themselves supposed she came from Cyprus. Hesiod's story is a very odd one. He related that Kronos, the son of Uranos, castrated his father and that the foam from which Aphrodite arose collected round the severed genitalia of the old sky god which had fallen into the sea. There was another story of the origin of Aphrodite, given by Homer, that puts her much later in celestial history as the daughter of Zeus and Dione. The later Greeks, in fact, distinguished between the motherless Aphrodite Urania and the more conventionally generated Aphrodite Pandemos, but there is no ancient authority for this. Cornford,[9] among others, has clearly shown that Hesiod's Theogony is derived from the ancient mythologies of the mainland of western Asia; such mythologies are apparently explanations of still older rituals. We may guess vaguely that behind the succession of Uranos, Kronos and Zeus lies a series of divine kings, mutilated or killed by their successors, and that the generative power of water and the abundance of the sea was somehow supposed to depend on such bloody practices.

Of all the objects that can be associated with Aphrodite, none is more significant than the cowry shell.[10] We use her name *Cypraea* for the type genus of the cowry family. Egyptian princesses, far before the Greeks existed as we know them, wore magnificent girdles of gold cowry shells which doubtless promoted fertility. In western Tibet the women still wore, at least until a few years ago, an elaborate ornament composed of a brass fret from which hung strings of *Monetaria;* this fret was given a name that specifically implied a visceral

[8] The writer remembers one owned by his grandmother, who was born in the 1840's, and suspects that this example had a whistle mounted at the end opposite to the bells.

[9] F. M. Cornford, *Principium Sapientiae*, Cambridge, 1952.

[10] See particularly J. Wilfred Jackson, *Shells as Evidence of the Migrations of Early Culture*, with Introduction by G. Elliot Smith. Publ. Univ. Manchester Ethnograph. Ser. II. 1917.

significance; the word for a cowry appears to mean also womb. It was worn on the right hip, Buddha having been born from the right hip of his mother. When a woman was wealthy enough, she would add further strings of cowries to her belt and might attach some of the large species of *Cypraea* in addition.[11]

If we now return to land and look for the terrestrial parallel of the cowry, we find it in the mandrake, the true old world mandragora, as the evidence collected by Rendell Harris[12] clearly shows. Mandrake roots were used in a way like cowries, hung around the waist to promote fertility. At this point, very unexpectedly, the hanged man enters, for the mandrake was believed to grow at the foot of a gibbet on which a man, preferably innocent, had been hung.[13] There is one classical reference to the innocent victim being Prometheus.

To pick a mandrake was very dangerous, in fact fatal, so the mediaeval herbalist recommended that the plant be loosened and then tied to the tail of an unwanted dog. The dog was to be enticed from the spot with a piece of meat. As it ran to get the meat, the mandrake was pulled out, giving a horrible shriek as it left the earth; the unhappy dog expired and the herbalist went off with his prize. The linking of a dog to the terrestrial expression of Aphrodite is very interesting, because it is reflected in the extraordinary story of the origin of Tyrian purple. This dye, a bromine derivative of indigo, is produced in the pallial glands of a number of species of *Murex* and *Thais* and perhaps of other genera of the Muricidae. It was used as a dye in various parts of both the Old and the New World. One may wonder how the process was ever discovered, particularly since the dye is present in the gland as a leuco-form which requires exposure to air and light for some minutes before the purple color develops. The ancients, however, knew that one day Hercules was walking with a young Tyrian lady along the beach. His dog bit at something and then began to foam purple at the mouth. The lady was so intrigued with the color that she challenged Hercules to obtain her a dress of the same shade. Hercules, being a practical man, went to the source of the matter and established the Tyrian dye industry. There is a very persistent myth about sharks, *canes marini*, guarding pearls; in some cases in which the pearl fisher is killed by the shark, it seems possible that we have a story of which the mandrake myth is a con-

[11] G. E. Hutchinson, *The Clear Mirror*, Cambridge Univ. Press, 1937. See also *Lakes in the Desert* (Amer. Sci., *37:* 399, July 1949) for a photograph of a western Tibetan lady wearing the ornament in question.

[12] Rendell Harris, *The Ascent of Olympus*, Manchester Univ. Press, 1927.

[13] It was apparently said that the plant was derived from the semen of the corpse, which completes the Uranos parallel quite diagrammatically.

fused and inverted version.[14] The pearls are certainly connected with Aphrodite and they are, as both Shakespeare and Mr. Eliot assure us, the drowned man's eyes.

We may conclude, therefore, from this brief examination of part of the collection amassed on the Enchanted Voyage, that, thousands of years ago, when ships first put out into the Persian Gulf and the seas about Egypt, many of the objects that could be hauled up were endowed with magical power. At first they were hard to obtain; the ships were small and sharks perhaps dangerous. Moreover, the magical powers sought had a strange history, being derived from a goddess who had originated through some violence done to a god, a god who through this hurt was a benefactor of mankind. It is hard to avoid a suspicion that the color of coral, and perhaps of Tyrian purple also, implies his blood. This remote and uncertain history has had an enormous influence on the creative ability of man. Traces of it survive in the folklore and in ornaments of many people. As the original stories became more remote in time they became more majestic and have influenced the greatest poets right up to our own time. There are many things that are obscure in this history, and some of them can only be illuminated by further studies of the organisms that carry the myth. The distribution of practices must be more closely compared with the distribution of such organisms. Biological oceanography has some part to play in the final elucidation of the histories of coral, cowries, Tyrian purple, and pearls. Our science may have wide peripheral meanings and may help to light up very deep recesses of the human soul. And so, with Prospero:—

> Now my charms are all o'erthrown
> And what strength I have 's my own
> Which is most faint: now 'tis true
> I must be here confined by you
> Or sent to Naples . . .
> . . . . . .
> Gentle breath of yours my sails
> Must fill, or else my project fails
> Which was to please. Now I want
> Spirits to enforce, art to enchant
> And my ending is dispair
> Unless it be relieved by prayer
> Which pierces so, that it assaults
> Mercy itself and frees all faults
> As you from crimes would pardoned be
> Let your indulgence set me free.

[14] G. Elliot Smith in Jackson, referred to in footnote 10.

~ *PHYSICAL OCEANOGRAPHY*

THE PLACE OF PHYSICAL OCEANOGRAPHY IN OCEANOGRAPHIC RESEARCH

By

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Talking here at Woods Hole about the place of physical oceanography in oceanographic research is comparable to carrying coals to Newcastle. In no other locality are gathered so many institutions engaged in marine research, and nowhere exist such unique opportunities for integrated oceanographic research.

Most of us probably know that the initiative to establish the Woods Hole Oceanographic Institution was taken by the National Academy of Sciences, which in 1927 appointed a special committee on oceanography. The mutual dependency of the marine sciences has never been expressed more lucidly than in the report of this committee, which was submitted in November 1929. This report had been prepared by the secretary of the committee, Dr. Henry B. Bigelow, and was published in book form in 1931.

Dr. Bigelow did not claim that his views were novel. They had, for instance, been stated in the first report of the International Council for the Exploration of the Sea (1902), a council which had been formed primarily for the purpose of applying oceanographic research to fisheries problems in the waters of northwestern Europe. It may seem superfluous to speak on a topic which has been dealt with repeatedly during the last 50 years, but I venture to do so for three reasons.

In the first place it seems that the necessity of co-operation between marine scientists cannot be stressed too frequently. It appears peculiarly difficult to achieve close contact between physical oceanographers and marine biologists, perhaps because they have difficulties in talking in terms which are mutually understood. This point was emphasized by Dr. Bigelow who, on behalf of the biologists, wrote: "So the unfortunate biologists, even if mathematics are to them a closed book, as is the case with too many of us, must perforce take as keen an interest as do his physical confrères, in the modern applications of mathematics to ocean dynamics and hold as high an appreciation of them." How difficult it has been to reach this goal

is evident from the fact that as late as 1951 the International Council arranged a special meeting devoted to the discussion of the place of physical oceanography, or hydrography as it is called by that body, in fisheries research. It appears, therefore, that no matter how obvious the co-operation between the different groups of marine scientists may be, the need of repeating and, even more, of demonstrating the value of co-operation is still present.

In the second place, I wish to underline the necessity of broad training. Every oceanographer, regardless of how narrow his speciality may be, should have some basic knowledge of the fields of all the marine sciences, partly because he ought to be acquainted with the terminology of his fellow workers and partly because he should be able to recognize results within his own field which have a bearing on problems of others or to know where he may obtain information that has a bearing on his own. I believe this demand for a broad training has been better met in this country than in any other, perhaps because of the rapid expansion of the science of oceanography which has taken place here and which has made it possible to design adequate courses for general training.

In the third place, I am speaking on the place of physical oceanography in oceanographic research because I like to do so. I am convinced, though, that my thinking has been greatly influenced by Dr. Bigelow's book of 1931 because I have not, I regret to say, been raised in an atmosphere of cordial co-operation between the marine sciences. On the contrary, in my own country and probably in most of northwestern Europe there has been a tendency to draw a line between physical oceanography and the other marine sciences, a tendency which may be illustrated by the fact that there the term oceanography is generally limited in meaning to the physics of the sea only. Thanks to Dr. Bigelow I have been convinced of the fallacy of placing one single branch in a separate position.

It is not my task now to deal with physical oceanography as an independent science or to dwell upon its application to other sciences than the marine. Instead I wish to emphasize that, among the marine sciences themselves, physical oceanography is in the unique position of being independent of the other marine sciences while serving as an auxiliary science to these. Conclusions based on studies of physical conditions may be confirmed by findings in other fields, but I cannot recall a single case in which the explanation of observed physical conditions are based on results in, say, marine chemistry, biology, or submarine geology. On the other hand, it is easy to find examples to show that specialists from these fields have to turn to physical oceanography to obtain explanation of observed conditions.

A chemist may say that this is an exaggerated claim and that the development of chemical methods and their application to the chemistry of sea water is an independent science. This, of course, is true, but if the chemist is to account for such features as the distribution of the so-called plant nutrients, such as phosphates and nitrates, he is no longer on his own but has to turn to both biologist and physicist. Let me take an example. Observations show that the upper layers of the sea are generally poor in phosphates and nitrates whereas the deeper layers are relatively rich. For an explanation of this feature it is necessary to consider biochemical processes. The upper layers are poor in phosphates and nitrates because these substances are used by the marine plants in building up organic matter, and the deeper layers are rich because the phosphates and nitrates there are brought back into solution when organic matter is decomposed.

In the Mediterranean, however, both deep and shallow waters are poor in phosphates and nitrates, and for an explanation of this feature it is necessay to turn to the marine physicist. He can point out that a great exchange of water between the Atlantic and the Mediterranean takes place through the Strait of Gibraltar, with Atlantic water flowing in near the surface and a nearly equal amount of Mediterranean deep water flowing out close to the bottom of the Strait. There is every reason to assume that these conditions have existed for such a long time that an approximately steady state has developed, meaning that the amounts of phosphates and nitrates that are carried in and out must be equal. Since the inflowing water is mainly surface water, it has a relatively low concentration of phosphates and nitrates, while the outflowing deep water also must be poor in the same substances. Similar considerations account for the fact that waters of the North Atlantic Ocean contain less phosphates and nitrates than those of the South Atlantic.

Turning to problems in marine biology, it is evident that physical oceanography is an indispensible auxiliary science in the field of ecology. Instead of expanding on this subject in general terms I wish to present a few specific examples.

The environment, as represented by the physical and chemical characteristics of the waters, is of immediate importance to the primary productivity of the different ocean regions. Observations which were undertaken regularly at weather ship "M" during the spring of 1949 throw light on some salient features. Weather ship M is located at 66° N, 2° E. Gr., i. e., in such a high latitude that the light during winter is too weak to permit any production of phytoplankton. The question then arises as to when one may expect the vernal blooming of phytoplankton to begin. When tackling this

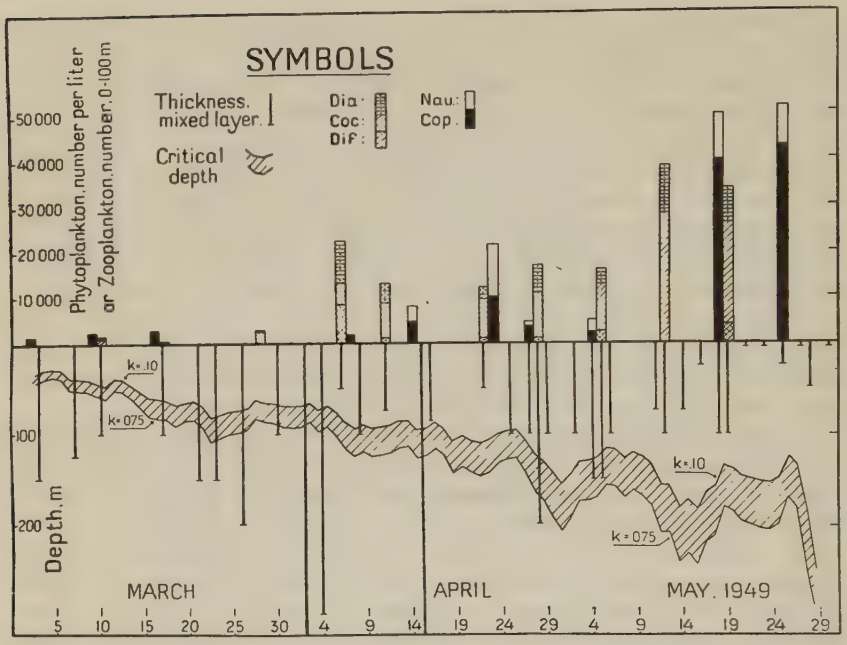

Figure 1. Results of observations at Weather Ship "M" (66° N, 2° E Gr.). Dia, Diatomaceae; Coc, Coccolithophoridae; Dif, Dinoflagellatae; Nau, Nauplii; Cop, Copepods.

problem one must bear in mind the fact that in these northern waters there develops during winter a thick, well mixed surface layer which, because of winter convection, is rich in plant nutrients. Gran and Braarud have suggested that production by photosynthesis cannot exceed destruction of organic matter by respiration if a deep mixed top layer exists. They reason that the plankton organisms within a well mixed layer are about evenly distributed; production takes place in the daytime only and near the surface where the light intensity is sufficient, whereas destruction takes place both day and night and evenly within the entire mixed layer. The total plant population cannot increase if destruction exceeds production. This implies that there must exist a *critical depth* such that blooming can occur only if the depth of the mixed layer is *less* than the critical depth.

With certain assumptions, particularly those concerning the extinction of light which travels downwards in the sea and the light intensity at the compensation depth, it is possible to compute the critical depth. The results of such computations pertaining to conditions at weathership M during March, April and May 1949 (see Fig. 1, lower part)

show that the critical depth increases as the season advances; that is, it increases as the intensity of the incoming radiation increases. The increase is irregular because of varying cloud cover. The critical depth is shown not by a single curve but by a band, because the exact value of the applicable extinction coefficient is not known. In Fig. 1 the varying thickness of the mixed layer is also shown by vertical lines which are entered for the days on which observations were made. On 7 April the thickness of the mixed layer was for the first time less than the critical depth, and only after the beginning of May did it become systematically smaller. The effect of intense surface heating, leading to formation of a very shallow mixed layer (stabilization), did not appear before mid-May. If our reasoning is correct, these features imply that the phytoplankton population should be expected to remain insignificant until the beginning of April and that a moderate blooming should start at the end of the first week of April followed by an intense blooming in the second week of May. The recorded amounts of phytoplankton which are shown in the upper part of Fig. 1 confirm these conclusions. These plankton collections also reveal that, simultaneously with the blooming of the phytoplankton, the number of copepods and nauplii increased. After the middle of May the copepods became so numerous that the increase in the phytoplankton population undoubtedly was checked because of grazing.

This example appears to demonstrate that the spring blooming of phytoplankton in high latitudes depends in a well defined manner upon the physical-chemical conditions of the surface layer, but unfortunately we have only an isolated example at hand, and the good agreement obtained may be fortuitous.

Next let us turn to a much more general problem, that of the relative productivity of different ocean areas, where by productivity we will understand the total amount of carbon that is fixed below each unit area of the sea surface. Assuming that productivity depends on the rate at which the plant nutrients of the surface layers are renewed and that the renewal takes place by physical processes, such as vertical convection, upwelling, and turbulent diffusion, it is possible to indicate roughly where large or small productivity may be expected (Fig. 2). Regions of high productivity should be found in high northern latitudes where winter convection operates and in lower latitudes where upwelling takes place, either along coastlines or in localities associated with diverging surface currents. In the Antarctic, high productivity should be expected because, in connection with the general circulation, deep water rises toward the surface. Moderate productivity can be expected in the vicinity of most coasts and at the boundaries of strong

Figure 2. Schematic representation of the probable relative productivity of ocean areas. Heavy shading indicates very productive areas, light shading moderately productive regions.

currents where irregular vertical motion may take place in connection with temporary disturbances. In lower latitudes the run-off from land after torrential rains may also be of great importance to the productivity of the coastal regions. On the other hand, low productivity is to be expected in lower latitudes in the central areas of the oceans where a renewal of the plant nutrients in the surface layer takes place mainly by decomposition *in situ* of organic matter or by slow diffusion from below.

As yet it is not known if these conclusions are valid. They are based on knowledge of the physical-chemical picture, but they assume that a net transport of plant nutrients to the photosynthetic zone is necessary in order to maintain a high productivity.

So far I have dealt only with features pertaining to the primary productivity of the sea. In these cases the relationships should be expected to be realtively simple, because the fixation of carbon is directly dependent upon the amount of light present in the surface layer and upon the available chemical substances; that is, it is dependent upon physical and chemical conditions. Different species of marine plants may react differently to the prevailing conditions, but in principle we have a relationship that can be fairly well defined. The situation is much more complicated when we turn to the animal populations of the sea. The distribution and behavior of the animal groups depend not only upon the generally unexplained reaction of animals to their physical surroundings but also upon their reaction to the biological environment in which they live. The higher up we go on the scale, the more complicated do the interrelations become, for which reason it is often difficult if not impossible to establish any clear-cut connection between the physical environment and the behavior of fish. However, certain more or less trivial relationships exist between the distribution of marine animals and the physical environment. I am thinking of the fact that the distribution of most species is related to temperature conditions; thus climatological charts of the oceans, representing average temperatures of waters and so on, also indicate the distribution of such species. Similarly, in recent years the occurrence of southerly species off northwestern Europe is associated with the temperature increase of the water during the last 25 years.

Explanation of these features is in the hands of physiologists and biochemists, but knowledge of the relationship of organisms to their physical environment can in some cases be used for forecasts of commercial value to fisheries. Let me take one example that is based on work by Dr. J. Eggvin, physical oceanographer at the Directorate of Fisheries, Bergen, Norway.

Off the coast of northern Norway there is a layer of coastal water

which has a lower salinity than the Atlantic water and which in winter also has a lower temperature. Below the layer of coastal water lies a transition layer within which the temperature in winter is 4 to 6° C. In late winter when the cod enters the Lofoten region for spawning, it is found in this transition layer. Therefore the depth at which the cod is encountered depends on the thickness of the well mixed upper layer of coastal water. When this layer is shallow, the cod is found close to the coast and in relatively shallow water, but if the upper layer is thick, the cod is found at greater distances from the coast and at a greater depth. In the former case the fishing is easier and simpler than that in the latter, and the yield from the fishing effort therefore depends on the structure of the water masses. Eggvin has been able to show that the thickness of the top layer depends upon such factors as precipitation, run-off and air temperature during autumn and early winter, and in some instances he has been able to issue forcasts of considerable economic value.

I shall confine myself to a presentation of these few examples (one being hypothetical) of relationships between the physical-chemical environment and the organisms. I know that most of you could easily enumerate many more.

To submarine geology, physical oceanography is an important auxiliary science, particularly when dealing with many problems in sedimentation. Let me mention that in November 1953 I met for the last time the late Professor V. Walfrid Ekman of Sweden, the Nestor among physical oceanographers. In his 79th year he had just published an exhaustive discussion of observations of currents which he had conducted with Helland-Hansen in 1930, but he had no intention of slowing down. He told me that he had become very much interested in the density currents which may explain the occurrence of layers of sand between layers of fine sediments at great depths and in basins and that he now planned to start a study of these currents. Others will have to do so now.

In conclusion I wish to return to the question of why it has been so peculiarly difficult to establish more intimate co-operation between specialists within the different marine sciences and why teamwork has been slow in developing within oceanography. I believe that the fundamental reason lies in the fact that, until recently, each specialist has had a limited and specialized training. I hope that this convocation in these surroundings with the most outstanding facilities for integrated oceanographic research will not only stimulate co-operation but will also stimulate efforts toward broadening the training of all students of oceanography.

STATISTICAL HYDRODYNAMICS[1]

By

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There are two major problems in the theory of hydrodynamics. One is its nonlinear equations. If a fluid motion is specified by giving its velocity at every point, one may inquire whether there is a second possible motion in which the fluid everywhere has just twice the previous velocity. The answer given by theory is "No." While this is simple, it is a negative result, and the problem requires a positive solution. The second problem arises because the observed motions of fluids are often highly irregular and random, typified perhaps by the gusts of wind that follow each other at irregular intervals or by the irregularity of successive breakers.

Many years ago, Osborne Reynolds (1884, 1896) attempted to make progress on both problems at once, and, in a famous investigation that involved both theory and experiment, he founded what has come to be known as the theory of turbulence. His ideas and methods have been fruitful, especially in the work of von Kármán and his followers (von Kármán and Howarth, 1938). However, the results obtained in this way have their limitations. For example, a result obtained in wind-tunnel experiments may be useful in the design of airplanes but may fail to be of much help to the meteorologist or oceanographer.

There is an increasing tendency to depart from Reynolds' path and to attack the two problems separately. The problem of nonlinearity dominates the recent attempts at forecasting weather numerically. While the results obtained are promising, they also indicate the difficult nature of the nonlinear differential equations. Much publicity has been given the high-speed computing machines that can perform millions of additions in a few seconds. When such machines are employed to forecast the motion of air masses over the eastern United States, their capacities are almost reached. And in these calculations, only some of the nonlinear effects are included while the effects of randomness are omitted completely. One says,

[1] Contribution from Scripps Institution of Oceanography, New Series No. 745a.

therefore, that turbulence is ignored in these calculations. This appears to be justifiable if one wishes only to forecast the movement of major air-masses 24 or 48 hours in advance.

There are other problems in which the reverse appears to be true. For example, the theory of surface waves indicates that nonlinear effects are subordinate. Two waves, one twice the height of the other, move with almost the same phase speed. This is not exactly true, but within the range of wave-heights encountered at sea, it is a good approximation. But it requires only a glance at the actual sea to be convinced of the randomness of the size, shape, and motion of surface waves.

Thus, an approach to these problems is indicated from the opposite direction: to neglect nonlinearity and concentrate on randomness. G. I. Taylor (1922) laid the foundations for such studies more than 30 years ago. At the present time, his ideas are strongly influencing the experimental attack on these problems. The term "autocorrelation methods" is probably familiar to many of you. These methods are now applied to analyses of wind-tunnel data obtained with the hot-wire anemometer. H. Panofsky is using them to analyse the natural wind. Here at Woods Hole, Seiwell applied them to ocean waves, and P. Rudnick has done the same at the Marine Physical Laboratory.

It is the theoretical rather than the empirical aspect of this approach that I wish to emphasize today. One can easily distinguish two very general kinds of random motion. The one is typified by the vertical motion of the sea surface over a point whose horizontal co-ordinates are fixed. The elevation changes, but its mean value is zero, being sometimes higher and sometimes lower. One may call this kind of motion a random oscillation—the variable oscillates about its mean value, but in a highly irregular manner.

The other type of motion can be illustrated by considering a small boat adrift at sea. If it is in the region of a steady wind, it will move down wind, and, on the average, it will move by equal amounts on successive days. But if it is in a region of light and variable winds, with no steady ocean currents, it will not show such a steady motion in one direction. Neither will it tend to return to its original position. It can be shown that its distance from the starting point increases (on the average) as the square root of the time but that it has no predictable direction. One may call this a random drift. In his highly original paper on such matters, G. I. Taylor used the phrase "diffusion by continuous motion" to refer to this phenomenon when applied to masses of fluid.

The theory of molecular diffusion had also encountered a similar

thing. This theory is based on the idea that in general the molecules of a gas move in straight lines with constant speed. This motion is altered abruptly by collisions with other molecules. Thus, the path of a given molecule consists of segments of straight lines, of random lengths and directions. In this case also, the distance from the starting point increases (on the average) as the square root of the time. The motion differs from the random drift in its segmental, discontinuous character: it has variously been called a random walk, or a random flight.

The distinction between the random drift and the random flight appears to be quite important, although not all problems are solved. In the kinetic theory of gases, it is shown that the random flights of molecules can be described by the so-called diffusion equation. It has often been supposed that this equation will also describe the random drift of water masses, and this has led to the well known concepts of coefficients of eddy-diffusion, mixing lengths, etc. Unfortunately, these concepts have, empirically, been unsuccessful in describing the motion of water masses in a quantitatively precise manner. At most, the diffusion equation can be supposed to yield a qualitative impression of the actual phenomena.

If one examines the theoretical derivations of the diffusion equation, one finds that they are all based on random flights of well defined objects. A water mass is not a well defined object and its motion is not a random flight in the sense of the above discussion. Moreover, it is difficult to modify the usual derivations of the diffusion equation so that they apply to this more general kind of random motion. One may therefore conclude that the failure to confirm the diffusion equation when applied to water masses is not a failure of the theory.[2] But this again is a negative result.

In order to describe the more positive results, I shall have to go into more detail. Norbert Wiener's (1949) great contribution to the subject has shown that a random oscillation always has a continuous power spectrum—and conversely. In one way, this is an old

[2] In the discussion at the Convocation, Professor E. Bright Wilson called attention to unsolved problems in this connection. Perhaps his remarks may be paraphrased as follows: the molecules of a gas execute random flights, but those of a liquid execute random drifts, since they are never free of the force fields due to neighboring molecules. Consequently, if the views advanced above are correct, one would expect the diffusion equation to be verified for gases, not for liquids. But, empirically, it is found to be equally applicable to both. This would lead one to expect that the diffusion equation would also govern the random drift of water masses, unless it can be shown that these are basically different from the molecules of a liquid. One such difference is their lack of permanent individuality.

result, familiar from the wave theory of white light. This, long considered to be a random oscillation, is known to have a continuous spectrum. However, Wiener's analysis opens the way to many new calculations and to a discussion of random drifts.

Let $x(t)$ be the co-ordinate of a point that executes a random oscillation. To this function, $x(t)$, there is associated a spectrum function $S_x(\omega)$, where ω is the frequency. This association is such that the area of the $S_x(\omega)$ graph is the time average of x^2, or, if one wishes, the square of the amplitude of the random oscillation. Now let

$$u(t) = (dx/dt)$$

be the velocity of the point; there will also be a spectrum function, $S_u(\omega)$, associated to $u(t)$; since the velocity will also oscillate randomly, this is given by the equation

$$S_u(\omega) = \omega^2 S_x(\omega).$$

The relation between S_x and S_u is shown in Fig. 1. It is important to notice that $S_u(\omega)$ approaches zero with a horizontal tangent, as ω approaches zero.

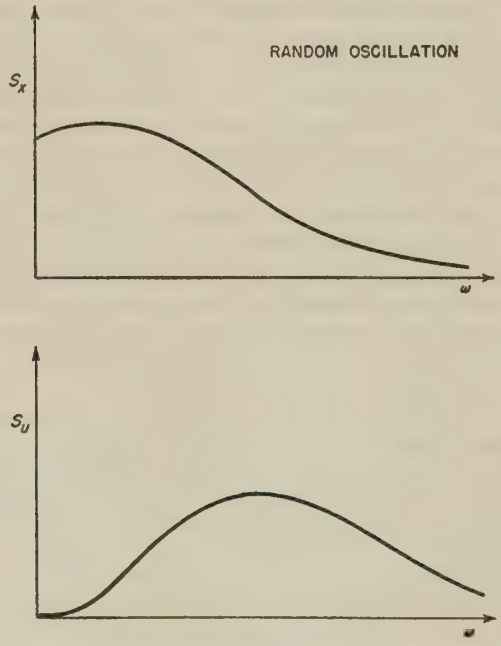

Figure 1. Relation between displacement and velocity spectra for a random oscillation.

Thus far I have been speaking of a random oscillation. Let me now turn to the random drift. In this case, the velocity still oscillates randomly, but $S_u(\omega)$ may not approach zero, or if it does, it may not do so with a horizontal tangent. If we now calculate

$$S_x(\omega) = S_u(\omega)/\omega^2,$$

we find that $S_x(0) = \infty$. The area under the $S_x(\omega)$ graph will then be infinite (see Fig. 2) and the amplitude of the displacement will diverge. This is precisely the characteristic of a random drift.

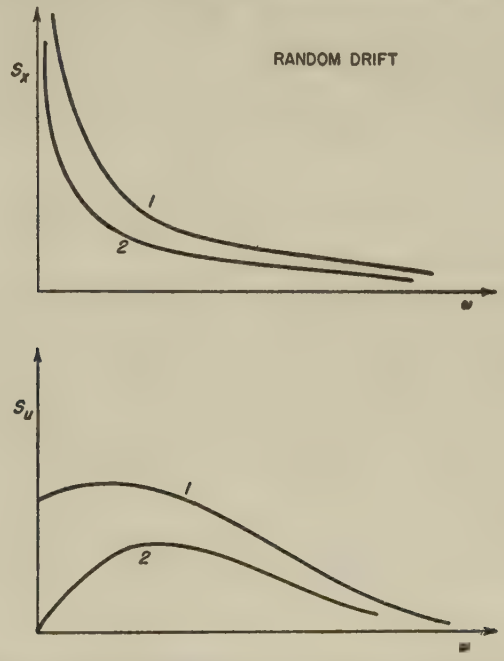

Figure 2. Relation between displacement and velocity spectra for random drifts.

We have thus learned to distinguish between random oscillations and random drifts by an inspection of the velocity spectrum. We see that the distinction depends on the low frequency components in the variation of the velocity and that these must be very small indeed if the motion is to be random oscillation.

Now, if one neglects the nonlinear effects, it is possible to make many calculations about random oscillations and random drifts. This becomes almost as easy as the corresponding calculations about simple harmonic oscillations. I shall summarize the results of three such calculations.

In each, it was postulated that the vertical displacement of the sea surface is a random oscillation, not a random drift. In each, the objective was to see if the horizontal motion could then be a random drift.

In the first calculation it was supposed that the ocean had infinite depth and was incompressible—had the same density at all depths. The usual approximations of the linearized theory of gravity waves were introduced. It was then found that the horizontal motions were everywhere random oscillations. It was impossible for horizontal random drifts to occur unless the sea surface was so violently agitated that the rms wave-height was infinite.

In the second calculation, the ocean depth was supposed to be finite, whereas all other approximations were retained. It was then found that horizontal random drifts were possible and obeyed the law

$$X = KZ\sqrt{(T/D)}.$$

Here

Z = rms height of the gravity waves,
X = rms horizontal displacement occurring in the time interval T,
D = depth of ocean,
K = factor depending on depth below surface at which X is determined, etc., etc.

From this one would conclude that random drifts should be more pronounced in shallow water than in deep, and this is probably in accord with the facts. This was not all that could be determined: One could consider the relative motion of two nearby particles of water and show that this must be a random oscillation. The two particles might drift, relatively to the bottom, but they could not drift apart.

In the third calculation, the water was supposed to be stably stratified, its density increasing exponentially with depth. It was therefore possible for internal waves to occur, in addition to the surface gravity waves. The latter caused horizontal random drifts governed by the same formula as that given above. The internal waves, however, caused random drifts governed by the formula

$$X_I = K_I\,(Z_I/D)\,\sqrt{T}\quad,$$

Z_I being the amplitude of the internal waves. The dependence on T—the time interval required for the rms displacement X_I —is the same, but the ocean depth enters in a different way. Moreover, if one now considers two particles, they may drift apart. This is so essentially because a stratified ocean has many modes of oscillation

while an ocean of constant density can oscillate only with surface waves.

These calculations have therefore isolated two factors that together can cause the random drift of water masses: the one is the bottom, the other is the density gradient. Neither, by itself, can cause drift.

I do not think that this is the only mechanism that causes the random drift of water masses in the ocean, but it is an especially simple one. Another mechanism is undoubtedly connected with steady currents, which, by themselves, would cause only steady displacements, increasing linearly with time. When these are combined with surface and internal waves, a random drift may be superposed on the steady displacement. The necessary calculations for this problem are not impossibly difficult, but so far they have not been completed.

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HIGH FREQUENCY SPECTRUM OF OCEAN WAVES[1]

By

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The subject "Motion in the Ocean," assigned to me by President Bronk, includes fish, Aqua-lung divers and density flows. For a very good reason I must confine myself to motion of the water itself. Further, I will limit myself by considering only that part of the motion of ocean water that can be associated with surface waves. It is nearly, but not quite, the irrotational component of the motion. This of course limits the subject, but not so much as you may think. It is by no means impossible that some processes previously explained by turbulence—the oceanographer's way of saying that he doesn't understand—are part of the surface wave problem[2] and that the Gulf Stream would be only a fraction of its present magnitude if the high frequency tail of the surface wave spectrum, the wavelets and the capillaries, were to be wiped out by an oceanwide slick resulting from a diatom bloom of impossible proportions.

The evidence for the latter statement was an experiment performed by Keulegan in a closed flume and repeated by Van Dorn in a 200 m long model-yacht basin. If TIDE, SURF, or some other detergent with a suitable oceanographic name, is sprinkled on a water surface over which a wind is blowing, the capillaries are quickly damped. This in turn has two effects: (1) The momentum transfer from wind to water (stress) is significantly reduced at all but low winds, and (2) the larger waves present, though not directly affected by the detergent, fail to be regenerated.

Apparently the tiny waves play a disproportionately important rôle in the problem of wind stress as well as in the generation of the more prominent parts of the wave spectrum. This was the beginning of a three year study.

The Spectrum. Let me first review what we mean by spectrum. Suppose we record against time the elevation of the sea surface above

[1] Contribution from the Scripps Institution of Oceanography, New Series No. 745b. being a report on progress in studies supported under a contract with the Air Force Cambridge Research Center.

[2] See Carl Eckart, Statistical Hydrodynamics, in this issue.

mean sea level, $z(t)$. If we use a tide gauge, we automatically discriminate against waves and swell in favor of the longer period (lower frequency) oscillation of tides. With a standard wave recorder we record waves and swell, but the instrument is rather unresponsive to capillaries, and perhaps to tides as well. Each instrument has its working range of frequency. Consider now an ideal instrument which is tuned with equal sensitivity to some narrow band of frequencies, from $f_0 - \frac{1}{2}\,\delta f$ to $f_0 + \frac{1}{2}\,\delta f$, but which does not respond at all to frequencies outside of this band. We now record the elevation of the sea surface (relative to mean sea level) through this spectral window. This gives us a wiggly record $z_{f_0}(t)$. The mean value of z_{f_0} is zero and is not interesting. The mean value of $(z_{f_0})^2$ is not zero and is interesting. It will be designated as $S(f_0)\,\delta f$, where $S(f_0)$ is the spectral density of the surface elevation (or simply, the spectrum) for the particular central frequency, f_0, to which our instrument was tuned. We now tune to a different frequency, f_1, and get a value of $S(f_1)$; by tuning to many such frequencies we generate the entire spectrum.

For our purposes it is more convenient to consider the spectrum $S(k)$ generated by an instrument tuned to a certain limited band of wave numbers k, where $k = 2\pi/\text{wave length}$. For example, the pitching motion of a ship is responsive to neither capillaries nor tides but only to wave lengths of the general order of the length of the ship. In actual practice the best way of determining $S(k)$ is to measure $S(f)$ and to make a theoretical transform. As a result of the work by Pierson and Neumann at New York University we have a fairly good idea of what $S(k)$ looks like (Fig. 1). It is peaked near

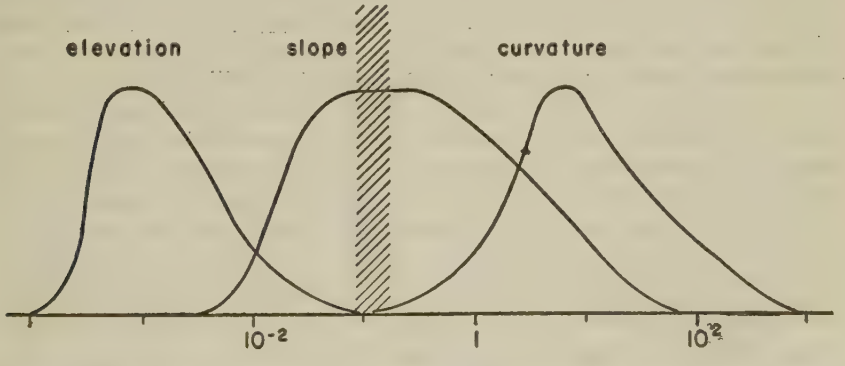

Figure 1. Schematic presentation of the spectra of elevation, slope and curvature as a function of the wave number k (units cm^{-1}). A slick has the effect of dissipating the parts of the spectra lying to the right of the vertical band.

$k = 5 \times 10^{-4}$ cm^{-1}, meaning that wave lengths of $2\pi/5 \times 10^{-4}$ cm, or about 100 m, contribute most to the spectrum. The contributions from waves longer than 500 m and shorter than 1 m are negligible. The total area under the curve is the contribution to the mean value of $z^2(t)$ from waves of all lengths, in other words, the mean square elevation, $\overline{z^2}$. The square root of this value, the rms elevation, comes close to what an observer would estimate to be the amplitude of the waves.

The two other curves in Fig. 1 are the spectra of slope and of curvature. The interpretation is as before. All we need to do is insert the word "slope" or "curvature" in the place of "elevation." It can be shown that the three spectra are given by

$$S(k), \qquad k^2S(k), \qquad k^4S(k), \tag{1}$$

respectively. Hence they are progressively weighted in favor of larger k's (i.e., short wave lengths, short periods, high frequencies) so that the study of slope and curvature provides a tool for learning about the high frequencies. The areas under the three curves are then interpreted as follows:

$$\begin{aligned} &\text{mean square elevation:} \int_0^\infty S(k)\, dk, \\ &\text{mean square slope:} \quad \int_0^\infty k^2S(k)\, dk, \\ &\text{mean square curvature:} \int_0^\infty k^4S(k)\, dk. \end{aligned} \tag{2}$$

These three integrals likewise provide a means of studying the spectrum with an increasing emphasis on high frequency. They are, in fact, the zero, second and fourth moments of $S(k)$, and if all moments were known, $S(k)$ itself could be reconstructed.

The procedure we have followed is to measure the mean square slope, and some work on curvature is now underway. The mean square values are easier to measure than their spectra. One needs only to record elevation (or slope, or curvature) by means of an instrument whose response is reasonably "flat" over the important range of the spectrum, square the output, and average. I shall now describe some measurements and what they have taught us about the high frequencies.

Sun Glitter. In the late summer of 1951, Charles Cox and I flew to the island of Maui, T. H., in a B-17 research plane provided by the Air Force as part of our Air-Sea Boundary contract. Gifford

Ewing's 58′ schooner, the REVERIE, went out under sail to make the necessary surface measurements, particularly of wind speed and direction. The principal goal was to measure mean square slope from photographs of sun glitter. The idea is simple. If the sea surface were flat calm, the glitter would be small; in fact, it would consist of a single undistorted image of the sun itself. But when the water is roughened there are thousands of dancing highlights within an extended glitter pattern. Each is a tiny image of the sun due to a reflection of a ray from an appropriately inclined water facet. The principle is quite old, having in fact been mentioned by Spooner in 1822 in *Correspondence with Baron de Zach.*

We have related the statistics of the slopes to the distribution of radiance in the glitter area. Our results, which I believe are more quantitative than those previously obtained, have been reported in a paper in which Cox is senior author.[3] Let me review the results briefly.

Distribution of Slopes. One result concerns directionality. It is possible to build up the mean square slope from contributions by waves of all possible directions, just as we had previously considered contributions by waves of different frequencies. When this is done it is found that, of those relatively short waves that are prominent in the slope spectrum, a substantial portion travels up to 60° to either side of the wind. This is a surprisingly wide beam into which the winds generate waves, a fact that must be taken into account in any theory of wave generation. The corresponding beam of the elevation spectrum, which features the longer waves, appears to be a bit narrower.

The slope distribution itself is nearly Gaussian, and, as is to be expected, the mean square values increase with wind speed. For a wind speed of 14 m sec^{-1} the rms slope is tan 16°. This means that there is a 10% chance that the slope exceeds 22° and a 1% chance that it exceeds 30°.

Since the distribution is nearly Gaussian, it can be represented by a single number, such as its mean square value. Cox and I found that the mean square slope increases *linearly* with wind speed. Now, if the elevation spectrum $S(k)$ is known, the mean square slope, $\int_0^\infty k^2 S(k)\, dk$, can be derived theoretically. Gerhard Neumann has proposed a particular form of an elevation spectrum which involves the wind speed as a parameter, and we find that it yields just such a

[3] Cox, Charles and W. H. Munk, 1954, statistics of the sea surface derived from sun glitter, J. Mar. Res., *13* (2): 198–227.

linear relation as we have observed. This is a satisfying result. It means that Neumann's lengthy observation of wave heights from the bridge of a German freighter plowing the Atlantic and our photographs of the sun's glitter in the Alenuihaha Channel yield consistent results. If I seem overjoyed by a consistency that is no different from that which should be expected and which is in fact found in other fields of physics, it is because most oceanographers have not been spoiled in this regard.

The linear relation is only part of the story. What about the proportionality constant? Here it is found that the rms slopes computed from Neumann's spectrum equal 60% of the values we observed. There are two ways of remedying the discrepancy, but I am not sure whether either of them, or the discrepancy itself, is significant. One is to fit Darbyshire's recorded data,[4] which are in some aspects superior to Neumann's observations, to Neumann's spectral function. The other is to compare Neumann's original coefficient with our observations in the presence of slicks.

The last remark needs some amplification. One of the duties of the REVERIE was to pump a mixture of diesel oil, used crankcase oil and sardine oil onto the water to make a coherent slick about one-quarter square mile in area. It was found that the mean square slope was reduced in this way by a factor of two or three, apparently by the elimination from the spectrum of waves shorter than a foot (Fig. 1). We have considered at some length why this should be so. We know, by the amount of oil added and by the presence of interference colors, that the slick was perhaps 1000 molecules thick. Reynolds' classical theory for monomolecular layers does not apply. The reason for the damping remains obscure, but the question *whether* oil dampens waves can be answered in a fashion. By removing only the high-frequency components, the mean square elevation is nearly unaffected, the mean square slope is reduced by a factor of two or three, and the mean square curvature is virtually eliminated. The optical properties of the surface are greatly altered, giving the appearance of smoothness seen otherwise on calm days only. I wonder whether this is not part of the reason why oil has been poured on troubled waters since time immemorial.

Curvature. The success achieved by the Neumann spectrum in predicting the distribution of slopes provides a temptation to extend

[4] Darbyshire's own fit, defective in the high frequencies, yields a *decrease* in mean square slope with increasing wind speed. It has been suggested by Deacon at the UGGI meeting in Rome that the discrepancy may be due in part to generation in shallow water.

it even further to inquire what it has to say about curvature. In doing so we realize that this means extending it far beyond the range intended by the author.

The Neumann spectrum predicts a mean square curvature which corresponds to a 12 cm radius of curvature and which is nearly independent of wind speed. Cox is now making measurements of curvature by sweeping a photomultiplier tube across a small area of sea surface and counting glitters. It can be demonstrated that the values so obtained can be used to compute the statistics of curvature. It is really too early to be certain, but it now appears as if the Neumann curvature is too low and that curvature does increase with increasing wind speed. Thus it may turn out that the Neumann spectrum is deficient in the capillaries and that we may find here a separate band of high activity.

It should be mentioned that the group velocity of waves reaches a minimum value of 17.8 cm sec^{-1} for a wave length of 4.4 cm. The minimum represents a balance, so to say, between surface tension and gravity. Shorter waves are principally affected by surface tension, longer waves by gravity. It is not unusual in geophysical problems to find prominent features in records to be associated with such a minimum in group velocity.

Dissipation. Another feature may make the study of slope and curvature rewarding. It can be shown that the energy dissipated due to molecular viscosity by wave numbers in the range $k \pm \frac{1}{2}\,\delta k$ equals

$$4\mu g\, k^2\, S(k)\, \delta k + 4\mu\gamma\, k^4\, S(k)\, \delta k \quad , \tag{3}$$

where μ is the molecular viscosity and γ the specific surface tension. Comparison with (1) shows that the two terms are proportional to the slope and curvature spectra, respectively. The total dissipation, due to all wave numbers, is therefore

$$4\mu g\sigma^2 + 4\mu\gamma\kappa^2 \quad ,$$

where σ^2 and κ^2 are mean square slope and curvature (eq. 2).

For a 10 m sec^{-1} wind we find $\sigma^2 = .06$. Blowing wind over Washington tap water, Schooley found $\kappa^2 = 1$ cm^{-2}. With these values, each of the two above terms equals approximately 2.5 ergs $cm^{-2}sec^{-1}$. If it should turn out that there is a separate band of high activity in the capillaries, then the second term would be correspondingly larger.

We have here an optical method for studying an energy sink of the ocean wave spectrum. Perhaps the energy is passed on by some nonlinear process (white-capping?) from the longer to the shorter

waves until it is finally dissipated by the high-frequency components. This would be analogous to conditions as found in the turbulence spectrum. The point of view is opposite to the one taken at the start of this discussion, when we pointed out that the growth of the larger waves requires the existence of the very short ones. Could it be that the capillaries provide for both construction and destruction of ordinary waves?

It should be pointed out that dissipation by capillaries is not the only way in which wave energy can ultimately be dissipated. Surface wave energy may also go directly into turbulent energy, by breaking or otherwise, and be dissipated finally by the small elements of the turbulence spectrum. Perhaps one can gain some insight into this aspect of dissipation by the following approach. It was pointed out to me by Norman Barber that the statistics of *acceleration* are closely related to the statistics of slope. Thus, for a wave profile $z = A \cos(kx - \omega t)$, the slope $\partial z/\partial x$ has an amplitude kA, the vertical acceleration $\partial^2 z/\partial t^2$ has an amplitude $\omega^2 A$, and, for the linear theory, $\omega^2 = gk$. Thus the accelerations at the surface, measured in units of g, equal the tilt in radians. For a 14 m sec^{-1} wind we found an rms slope of tan 16°; accordingly the rms acceleration is about $\frac{1}{4}\ g$. There is a 1% chance that it exceeds $\frac{1}{2}\ g$. If one dares to extrapolate this distribution even further, one can compute the probability that the acceleration exceeds g. Spray would certainly be formed if the acceleration exceeded g. There are obvious difficulties with this approach, but in some ways I find it more attractive than a discussion based on conventional breakers with 120° cusps. As an aside, what is the effect on micro-organisms near the ocean's surface when they are being tossed around with an rms acceleration of $\frac{1}{4}\ g$?

Wind Stress. We now return briefly to a problem mentioned at the beginning of this paper, namely the transfer of momentum from wind to water. At the Rome meeting of UGGI, Charnock gave a critical summary of reliable observations and concluded that this transfer increased with wind speed U faster than U^2 but slower than U^3. I think our results are not inconsistent with this conclusion.

If spray and rain can be neglected, the transfer of momentum is accomplished by two processes. First there is a transfer by *skin drag* as it is found whenever fluid flows over polished plates. It equals $\mu\ (dv/dz)_{z=0}$ either in the water or the air, with the shear dv/dz evaluated at the very surface. There have been no systematic attempts to measure dv/dz close to the surface. I am impressed with a casual observation by Kittredge. Working from a skiff, he put some dye on the water and then sprinkled a bit of talcum on the dye. He

found that the talcum drifted apart from the dye mark at a rate of something like 10 cm sec^{-1}. If we set the thickness of the dye as 1 mm, then the shear must have been of the order

$$\frac{10 \text{ cm sec}^{-1}}{0.1 \text{ cm}} = 100 \text{ sec}^{-1} ,$$

and μ times this quantity equals 1 dyne cm^{-2}. This is of the expected magnitude for the shear stress.

In analogy with other experimental evidence one can expect the skin friction to be proportional to U^2. In addition there is a transfer due to *form drag* resulting from systematic differences in pressure on the windward and lee sides of waves. Air impinging onto a vertical surface exerts a pressure ρU^2. If the slope is zero, the pressure is zero. The simplest interpolation between these values is then $\rho U^2 m$, where $m = \sin\theta$ and θ is the tilt in the direction of the wind. So far this is equivalent to Jeffreys' sheltering theory. The horizontal component of this pressure is $\rho U^2 m^2$. If the tilted surfaces move downwind with a velocity C, the appropriate expression is $\rho\ (U - C)^2\ m^2$. Over land, $C = O$ and m^2 is constant. The form drag accordingly varies as U^2. Over water, if $C \ll U$, then the average value of the above expression is $\rho\ U^2\ <m^2>$, where $<m^2>$ is the mean square slope component. Our results show that $<m^2>$ increases linearly with U, and hence the form drag varies as U^3. To allow for C, one is tempted to write the above expression in the spectral form

$$\rho \int_0^\infty [U - C(k)]^2\, k^2\, S(k)\, dk \ .$$

The integrand represents the contribution to the total form drag by wave numbers in the interval $k \pm \frac{1}{2}\,\delta k$. The integration can be carried out for the Neumann spectrum in closed form. One finds that the drag again varies as U^3 and that the resulting formula can be made to fit Van Dorn's and Charnock's results.

I won't make too much of this circumstance because of the many uncertainties involved. At what elevation is the wind to be measured? What is the effect of the lapse rate? Can the momentum transfer (and pressure pattern) for each wave number be assumed independent of other wave numbers, as implied in the foregoing integral? Can skin drag be added to surface drag? Is $C(k)$ the phase velocity, or does it involve also the orbital velocity of the larger waves present?

But, regardless of these difficulties, I am convinced that the form drag is much more closely related to the slope statistics than to the elevation statistics. I believe this conclusion is a departure from the

usually accepted view. There one deals with Reynolds numbers, or with roughness coefficients; and in either case, a length parameter is involved which is supposed to be mysteriously related to wave height. If the elevation statistics were essential, then the effect of a six foot sea on form drag would be equivalent to that of a six foot tide! If this were so, one would expect a critical dependence of wind stress on fetch and no measurable effect due to a detergent spread on the water surface. On the other hand, if the slope statistics, hence the high frequency spectrum, is the essential element of surface roughness, then the dependence of wind stress on fetch should be slight, as observed; and the reduction by a slick should be considerable, again as observed.

Before leaving this subject I should like to bring up one more possibility. The glitter measurements have shown that the wave spectrum is symmetrical with respect to wind direction, as expected. But under special conditions this directional spectrum might be skewed in the sense that waves travelling to the left of the wind would be more prominent than those to the right, or vice versa. This would give rise to a crosswind component in form drag. Henry Stommel has made the interesting suggestion that attenuation of high-frequency waves by slick bands lined up with the wind would result in just such a directional skewness and that this would give a component of drag into the slick from both sides of the slick. There would then be a convergence in flow which might serve to maintain the slick in the compressed state prerequisite to wave attenuation.

Comments on the Statistical Approach. By this time some violent objections to our statistical point of view will have developed in the mind of the listener. If he is a plant taxonomist he is likely to ask where we would stand today if our knowledge of diatoms and their wondrous diversity consisted of statistical descriptions of their geometry. Are we obscuring observable quantities behind statistical curtains? Certainly "mean square slope" and "mean square curvature" are not readily identified with anything we can see on the sea surface. And even if it is admitted that some statistical treatment is required, why choose probability distributions and spectra among the many possible statistical descriptions?

I really don't know of a good answer to these objections, but at least a few things can be said in favor of our choice. Suppose you were to ask me about wave conditions off La Jolla in June and I were to reply by presenting you with file cabinets filled with records of wiggly lines. I don't think this would be quite satisfactory. Apparently some kind of statistical generalizations are in order. But

which are the most pertinent? This depends entirely on the problem to be solved, and for a great variety of problems the spectral description is convenient. Pierson and St. Denis have made remarkable progress in studying the motion of ships in a rough sea by first decomposing the sea surface into a spectrum of elementary sine waves of all possible lengths and directions and then considering the response of the ship to each of these. The essence here is that the response of the vessel be *linear*, i.e., that the total response should be the sum of the responses to the individual trains; this is pretty much the case so long as there is no green water over the bow. An example for high frequency waves is the scatter of radar and sound waves from a rough sea surface. This can be estimated from a knowledge of the wave spectrum. The only components of the wave spectrum that are important here are those whose wave lengths are of the same order as the wave lengths of the radar or sound waves. For present equipment this means the high frequency tail of the spectrum.

However, our principal interest is not to compute the scatter of radar waves, even though this is a matter of some importance, but to work toward a respectable theory of the generation and decay of ocean waves and of the transfer of momentum across the air-sea boundary. How does the spectrum help here? In other fields of physics a knowledge of the spectrum has helped a lot in pointing the way toward a successful theory. If at some future time a theory of wave generation should critically confirm the Neumann spectral function, such a theory should be considered a major achievement.

On the other hand, it is certainly dangerous to select one's statistical tools without having first had a good look at observations uncontaminated by analysis. Fig. 2 shows a remarkable photograph taken by von Arx off the Woods Hole dock. In spite of considerable effort I have never been able to improve on this picture. It shows a series of tiny wavelets on the lee side of larger waves, with the shortest wavelet farthest out in front (downwind). On days with light winds, wavelets can be seen by the naked eye. Wavelets are eliminated by a slick, and I suspect, therefore, that they represent a major contribution to the statistics of curvature. However, what may be the essential features of wavelets, their dispersive appearance and their location relative to the crests of the longer waves, could be buried by routine measurements of curvature. To put these features into an appropriate statistical language requires more elaborate statistics. I think this photograph indicates how important it is to look at the raw data before deciding on pertinent statistical parameters.

I have a simple hypothesis as to why these dispersive wavelets are

Figure 2. The wind blows from right to left.

found only forward of the wave crest and why the short waves lead the long waves. Consider (Fig. 3) an underlying long-crested wave with height H, phase velocity C, and orbital velocity U (less than C). Capillaries with phase velocity c ride this wave. Their total rate of advance is $c + U$, and they will remain stationary with respect to the wave crests provided

$$C = c + U(x) \ . \tag{4}$$

Consider first the region (1) to (2) forward (to the right) of the crest. If $c + U$ were to exceed C slightly, the capillary would advance to a region of lesser U until eq. (4) is again satisfied. If $c + U$ is slightly less that C, then the capillary falls back toward the crest where the orbital motion is faster, and equilibrium is again achieved. However, in the region (2) to (3) behind the crest, a slight variation from the stationary condition leads to conditions for the permanent

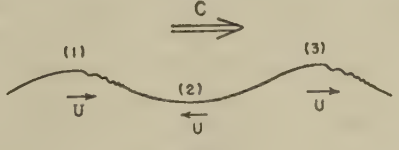

Figure 3.

removal of the capillary from this region. Stationary conditions exist only forward of the crest, and to the rear of the trough. This is in fact where the capillaries are found.

We now wish to find the capillary wave lengths. The simplest case is one for which $L \gg L_m$ and $l \ll L_m$, where L and l are the lengths of the waves and capillaries, respectively, and where $L_m = 1.7$ cm, the wave length for minimum phase velocity. It can then be shown that

$$l = \frac{{L_m}^2}{L}\left(1 - \pi \frac{H}{L} \cos \frac{2\pi x}{L}\right)^{-2} \ ,$$

where x is the distance forward of the crest. Since $H \ll L$, this means that l is largest near the crest, then diminishes forward. Qualitatively this agrees with what is observed.

There is an alternative, and perhaps an even simpler hypothesis. Suppose one takes a straight edge (thin compared to L_m) and brushes it across water at a velocity exceeding the minimum phase velocity (23 cm sec^{-1}). Then one eventually finds a series of capillaries preceding the straight edge and gravity waves following it, with each crest remaining parallel and at a fixed distance from the ruler. The

short waves are in front because their energy is propagated faster than the wave crests. The reverse holds for the relatively long waves to the rear. This is the one-dimensional equivalent to the "fish-line problem," and it is fully discussed by Lamb (p. 270–271, 6th ed.).

It is not unreasonable to assume that, at the very crests of waves, where instability first manifests itself, such a travelling stress is applied. If this happens over only a short period, then there will be noticeable dispersion, with the shortest waves farthest in front. In this sense, the capillaries on the photograph may represent a shock front in a dispersive medium.

RECENT ADVANCES IN OUR UNDERSTANDING OF THE CIRCULATION PROBLEM AND THEIR IMPLICATIONS[1]

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When I was first asked to prepare a paper for this program, although it was obviously an honor, I hesitated to accept, for it seemed to me doubtful that a regular staff member of the Woods Hole Oceanographic Institution should be asked to express his views. After all, one of the chief purposes of this Convocation was to gain the benefit of your collective advice as to future scientific policy. The occasion of the dedication of the new laboratory was to be used to give us a new start and some encouragement to embark on novel, and perhaps more fruitful, directions. For nearly 25 years I have been intimately associated with our efforts to advance the circulation problem. I have written or helped to write many annual reports. What could I possibly say here that would be new or helpful to the main purpose of these meetings?

However, after considerable hesitation I came around to the idea that at least one paper in this series might well review the factors on which our present program is based and might try to set forth our plans and expectations for the future. If we are to gain the benefit of sound advice, those who have been asked to help us should at least know what we now think the problems are and how, until now, we have been planning to meet them.

Under the term circulation problem I include a wide variety of phenomena, both large scale and small scale. It has been emphasized many times that the movements of the water constitute the central theme in oceanography and that we are justified in considering this as a separate and somewhat specialized field of science. Few aspects of oceanography can be successfully divorced from the consequences of water movements, either the slow ones or the swift ones.

In 1931, when the ATLANTIS became available for full time operation, the underlying reasoning of the geostrophic equation was just becoming generally understood. It seemed that here at last

[1] Contribution No. 765 from the Woods Hole Oceanographic Institution.

was a way to make physical oceanography three dimensional and at the same time put it more nearly on a synoptic basis. When our field work was planned, our first idea was to follow the highly successful lead of the International Ice Patrol Service and compute water movements from networks of discrete observations of temperature and salinity. If we could only occupy a large enough number of hydrographic stations, we could then solve for the water movements over a considerable area. In those days the circulation problem seemed just a matter of patience, hard work at sea, and arithmetic. Because the Gulf Stream was near at hand and because it was obviously a major factor in the circulation of the North Atlantic, we began collecting temperature and salinity profiles across this exceptionally swift current when the ATLANTIS was ready. We had little comprehension of the magnitude of the task and of the limitations of the then available observational techniques.

The first few Gulf Stream profiles seemed to agree rather well with the simplifying assumptions of the geostrophic equation. Computed surface velocities on the whole agreed with available navigational experience, but even after several years of sailing back and forth across the Gulf Stream we had gained little comprehension of how crude our navigation was. The interpolation between the points of observation of temperature and salinity smoothed out the picture of the current system to about the same degree as the necessary averaging of the surface current, as observed by celestial navigation. In neither case were we able to see any of the fine-scale structure that today makes the circulation problem seem so much more complex.

It is amusing to note that in 1934, after about three years' work in deep water in the western North Atlantic, we considered that the ATLANTIS had gathered enough data on the Gulf Stream System and that we should resume systematic surveys of the Gulf of Maine, where the interests of biologists were traditionally centered. It was a rude shock when, after nearly two years of monthly surveys of the Gulf of Maine area, the physical oceanographers here could not even write one paper that added anything of significance to what Dr. Bigelow had worked out on the basis of much less systematic and more widely spaced observations. Although we came to realize in this way that the circulation problem in shallow water is an extremely difficult one, we still thought that in deep water the movements were broad and relatively steady and that they could be generally explained on the basis of the then available theory.

Thus, in 1937, the ATLANTIS returned to making Gulf Stream profiles, this time on the line between Montauk Point and Bermuda. Internal waves became somewhat of a worry, but if enough stations

were occupied these could be smoothed out. We began to encounter more and more "eddies" on either side of the Gulf Stream. Usually these involved transports equivalent to the Gulf Stream, but sometimes they were considerably weaker. Where do eddies leave off and internal waves begin? This became the doubt which indicated that all was not well.

At this point World War II intervened, and for four or five years the circulation problem was neglected. On the other hand, during the war years we acquired several new techniques, but, perhaps more important, we attracted some new people. As most of you know, after the war, under the leadership of Mr. F. C. Fuglister, an entirely new approach to the problems of the Gulf Stream System was developed. By exploiting the capabilities of the bathythermograph and of Loran navigation it became possible to gain measurements of the surface velocity and at the same time observe approximately the temperature distribution in three dimensions, at least down to moderate depths. At once the Gulf Stream became surprisingly narrow and swift. The theoreticians, led by Mr. Henry Stommel, quickly advanced a plausible explanation. The effects of the change of the Coriolis force with latitude were for the first time included in the theory. But after a year or so of further field work to the eastward, it became evident that the currents were not only narrow and streaky in the west, where they have a northward component, but that they also retain these characteristics in flowing from west to east.

It soon became evident also that for the type of survey so successfully initiated by Mr. Fuglister a single ship was inadequate. To gain a picture of the large scale pattern of the Gulf Stream System, several vessels should be used simultaneously. Operation Cabot was organized and that more or less terminated a stage in our thinking. It is the nearest approach to synoptic deep sea oceanography that has yet been attempted, but it is now abundantly clear that even six ships are too few to gain a clear picture of a phenomenon as large as the Gulf Stream.

In retrospect, Operation Cabot had some other serious deficiencies. It was planned with a preconceived idea of what would be encountered. It concentrated on what might be called "Gulf Stream Number One," and only one of the ships obtained a few observations in the overlapping "jetlet." From this and other experiences, Fuglister's multiple current hypothesis was born. It would require a survey considerably more elaborate and extensive than Operation Cabot to demonstrate conclusively the existence of the sort of current system that he has postulated. A major question today is whether or not

such a survey should soon be attempted. If so, are we ready for it and should we again survey a considerable area of the Gulf Stream System or should we focus attention on some other major current? After all, the Gulf Stream may turn out to be a special case. There is growing evidence that the North Atlantic is not a typical ocean. The reconnaissance phase of North Atlantic physical oceanography can be considered completed. Should we continue to study these relatively well explored currents or should we turn our attention to a contrasting region?

Opportunities for multi-ship surveys will not come frequently. We should plan the next one with the greatest of care. Were precise navigation available, there would be little doubt in my mind that the next major effort should be in the Antarctic Circumpolar Current, for I believe that the so-called convergences are nothing more than overlapping jet streams. Yet, without something comparable to Loran navigation, a co-operative survey in southern latitudes might fail. Nevertheless, this is surely the only ocean current system that is relatively free of continental boundaries.

Since Operation Cabot, we have been casting about for new techniques that would be helpful in gaining a better understanding of Gulf Stream phenomena and we have also had a good look at the currents in the trade wind latitudes. I will elaborate on some of the new techniques, but the best that can be said of our efforts in low latitudes during 1952 and 1953 is that they were inconclusive. For lack of precise navigation, the surveys were too open to yield a unique solution. Mr. Fuglister's principle that the current always appears to follow the ship was once again in operation. How to contour temperature data gained in the course of widespread surveys is still a major problem in physical oceanography. The most direct and simple solution to the contouring problem may yield most misleading results, as he showed recently.

Several years ago Dr. Willem Malkus made a direct attack on the old problem of how velocity varies with depth. How deep is the Gulf Stream and how does the velocity profile change in the downstream direction and how does it differ in landward and seaward meanders? He has evolved a variety of devices which we call bathypitotmeters. The development stage has just about been completed. New tactics have evolved, and the first cruise using these new techniques has just terminated.

It is too early to predict what will come out of these new methods, but at least it can be said that they should help to fill a long felt need. Dr. Malkus has set himself the problem of observing how the velocity structure in a given water mass changes with time. He simply

drifts with the current, observing the crosscurrent components, the path of the axis of the current, and the velocity-depth changes. For the remainder of this summer we plan to monitor the Gulf Stream almost continuously in these and other ways. What happens to the end of the current after a meander has broken off? How long does the resulting eddy persist? These are critical questions and I do not see how to proceed until they are answered.

Meanwhile, the hope that a plane can monitor the broad synoptic situation while a ship examines a limited part of it in three dimensions is being partially realized. Dr. William S. Richardson has greatly improved the radiation thermometer, first devised by Stommel and Parsons. He has flown it many miles, and what he sees of the temperature pattern of the sea surface adds greatly to our present difficulties. While the ships have shown that the Gulf Stream is discontinuous on a large scale, the plane seems to indicate that, at the surface at least, it is also discontinuous on a small scale.

As most of you know, a few years ago Mr. W. S. von Arx took a new and radical tack. Our vessels are confronted with the difficulty that the ocean is too big. He proposed to make a small one. He started very small indeed, for his first model was no larger than a good sized salad bowl. He is now planning one 24 feet in diameter. It would be feasible to operate a model 100 feet or so in diameter and still spend less money than that consumed by a single vessel. There is little doubt today that model techniques in the earth sciences are embarking on a rapid development. The model integrates much more easily and more cheaply than electronic computors, and it is much more fun to watch.

The future of the circulation problem would be bright were it not for the uncertainty that all important factors operating in the prototype could be included in a reasonably sized model. If the model has to be 100 yards in diameter rather than 100 feet, then we are out of luck. Meanwhile, the model technique will clearly be helpful to the development of somewhat simplified theories of oceanic circulation, and it should also show the critical areas in the southern hemisphere where field observations should be concentrated.

It is my belief that the model technique constitutes an essential link between field observations and theory. Only by using all three approaches simultaneously and wisely are we likely to succeed.

Now, what can be said about the future of theories? We have some very good and satisfactory theory that is based on the idea that the average winds over the ocean provide most of the energy necessary to maintain the horizontal currents. Theories that include the obvious major effects of seasonal production of large amounts of surface water

of different densities are just being born. Theories that can deal simultaneously with thermo-haline processes and a variable wind field have yet to be devised, although it is clear that in the ocean we have currents that range all the way from pure wind currents to currents deriving all of their energy from density differences. Most currents appear to be of an intermediate character, and it may be many years before we can assign values to the relative importance of these two causes of the circulation.

Meanwhile one very important lesson has been learned in shallow water. After the war, when interest was developing in estuarine circulation, rapid progress was made because in general a continuous record of one factor was available, namely the freshwater inflow. The analysis of the shallow water surveys had to account for the varying contribution of energy from the river. We were no longer dealing with spot observations alone. The spot observations had to be so analyzed as to transmit a measured amount of fresh water out of the estuary. Undoubtedly our deep water observations could be made to yield more convincing results if they too had to allow for some continuingly measured variable.

Several recent efforts in this direction have produced encouraging results. Mr. Wertheim's electrodes on either side of the Florida Straits presumably give a record of the energy available to the beginnings of the Gulf Stream. Changes in the initial energy should be reflected in future happenings downstream. We are dealing with a system that has both short period and gradual variations. Our attempts to analyze synoptic data must admit to this fact and should be directed so as to advance our ability to predict what will happen off New England, for example, following radical changes in the Florida Straits.

In this respect I believe we are somewhat ahead of meteorology, for we have stumbled on means of getting continuous measurements of the variations in energy available to parts of the marine system. I feel that a considerable effort to gain continuous data on oceanic circulation is well justified and will yield increasingly valuable results, especially after really long records become available.

The long term approach is really the responsibility of the institution rather than of the individual researchers on its staff. Few of us have the patience to wait for accumulation of even a 10 year record. The Woods Hole Oceanographic Institution has now been in existence nearly 25 years, yet today we do not even have a continuous record of winter temperature in Woods Hole Harbor for the whole of this period.

Only recently, through Mr. Stommel's enthusiasm, has the idea

of a continuing oceanographic observatory been born. No doubt he will soon turn his attention to other matters, yet it is clear that some organized effort to secure long series of data will in time pay big dividends. More of our present effort should be directed toward a selection of critical places where continuous observations will yield the most significant results and also toward methods of devising inexpensive means of recording a number of types of information simultaneously.

Too few oceanographic instruments that can obtain long continuing records comparable to those of a tide gauge ever become fully developed. The inventor, in satisfying the needs of his particular research problem, nurses the instrument so long as it is critical to his particular study and then puts it on the shelf. We have no organization at present to refine the design of such instruments and to put them to work gathering long series of data that may become of great interest to future investigators. This has been a serious deficiency in our past program and I hope that something can be done about it in the future. The idea that an oceanographic laboratory should maintain a few permanent points of observation in the ocean seems to me a sound one and it is high time that suitable equipment was developed.

To summarize: our experience indicates that it will be a long time before we are able to develop adequate synoptic pictures of the circulation pattern of considerable areas of the ocean in three dimensions. The more detailed our surveys, the more difficult the problem has become. The hope is that models can be developed that can be trusted. Our ships, supplemented by buoys and submerged recorders of various kinds, can observe what goes on at a given spot or in a given water mass as a function of time, and perhaps the model can interpolate between these few points of continuous observation. The outstanding characteristic of oceanic circulation is that it is variable over a very wide time scale. It varies significantly from week to week, from month to month, from year to year, and probably over periods of 100 years or so as Mr. L. V. Worthington has recently pointed out. Combining observations widely separated in space and time usually obscures the very factors we are trying to understand.

We still have exploratory survey work to carry out in large areas, especially in the southern hemisphere. Before this is undertaken on any considerable scale I feel that we should first develop a means of precise navigation. To survey new areas by using conventional techniques perhaps would seem satisfactory to a geographer. It would fill in areas that are now more or less blank on our charts. But so far as the circulation problem is concerned, I doubt that we

would learn much that is new, except possibly through multi-ship operations. Meanwhile, awaiting a better and more universal means of navigation, I believe that we should continue to study the limited areas where Loran navigation at least is available.

When one looks back on the history of the circulation problem, advances in our understanding of a large scale phenomenon has always depended on major improvements in navigation. Maury's contributions depended on the chronometer coming into general use. He developed the climatological, two dimensional ocean that was the starting point of modern oceanography. The next hundred years were consumed in trying to understand a three dimensional, statistical ocean. Only when Loran navigation became available did we begin to appreciate the pitfalls of the averaging and interpolating procedure. We need something much better than Loran and we need to have all parts of the system under our control. If any of you doubt this statement I suggest that you spend a few hours aboard the ATLANTIS while she is tied up to the dock. You will find that Woods Hole moves in sudden and unpredictable ways as some technician at Halifax or Nantucket decides that it is advisable to alter the timing between the master and the slave station.

At present our only means of proving out either developments in theory or in models is through navigation. Until we have improved navigation by another order of magnitude we will remain in danger of confusing the average ocean with the one that exists at any one time. In this respect meteorologists are ahead of us, although in the long run we have an advantage, for our system changes so much more slowly.

OCEANOGRAPHY AND ENGINEERING[1]

By

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I have been greatly honored by the invitation to this gathering and to the dedication of this newest and most impressive implement to the study of the province of oceanography.

I wish that I could unveil some blinding breakthrough to shine by its own light in this assemblage. However, I present only a few humble rays that interrelate two great fields of human effort, Oceanography and Engineering, and the light engendered thereby is scarcely adequate to view their raveling. It seems happily appropriate to me to discuss these relations, virtually within sight of this laboratory which we might consider as being recognition of the value and implications of oceanography to a great branch of engineering, Naval Science. In this discussion I hope to indicate several ways in which Engineering and Oceanography are related and to discuss what I consider the most important mutual problem of these two provinces, the need for rational realistic co-operative attack on complex problems.

Certainly I do not need to point out the many ways in which the information of the oceanographer is of importance to the engineer who is involved in marine problems. Such lack of exchange of information as does exist is being reduced rapidly as the engineer becomes better acquainted with the reality of the oceans and becomes better able to ask questions. Recent work on west coast sewer outfalls is an example. Engineers, troubled by the stability of a surface layer of light effluent from their great outfalls, have, with the aid of oceanographers, planned to effect mixing of the effluent below the thermocline and thereby avoid surface stability. Many such examples could be enumerated. Recognition of the variability of ocean waves in ship-model studies is another valuable step toward realism.

In the application of engineering information and methods to oceanography, the record is not so commendable. Certainly much of the engineering information on models, evaporation, turbulence, settling, jets, and mixing is amenable to generalizations, with immense implication to studies of oceanographic processes. A type of problem

[1]Contribution from the Scripps Institution of Oceanography, New Series No. 745c.

that springs to mind is that of the eventual history of the downwardly projected mass of water in a deep-sea comber. Can its momentum be balanced short of the thermocline? Is it important in the deepening and steepening of the thermocline?—in mixing?—in friction? Surely there are many engineering investigations on jet mixing that will increase evaluation of the importance of such possible processes. Woods Hole scientists have employed engineering methods in their important investigations of oceanic circulation. Others, including myself, have applied engineering model laws to such matters as the motion of sea animals, with highly suggestive results. For instance, in studying the model laws of a simple prey-predator relationship, I am convinced that the factors which limit the size of predatory pelagic fishes are demonstrable, that many outstanding characteristics of fish form can be explained, and that probably a strong evolutionary requirement for the development of rapid changes in the schooling habits of pelagic fishes can be demonstrated.

I believe that there is great potential value in exchange of information and methods in this direction and that this should be fostered. However, the fact that one should consider it necessary to suggest a mere exchange between oceanography and engineering points up a far more important and extensive problem in the relationship of the "pure" and "applied" fields. I have felt that here a grave gap exists in our methods of progress toward understanding. To enable me to point out this problem, why it has arisen, and why it is seldom recognized, I will follow, for what they are worth, some contemplations which begin with the hierarchy of man's endeavors to discover the truths of the world.

May we not say that all of man's effort to understand the universe stems from a need to feel more secure within it? I believe that this is so and that the sense of security is of two kinds. The first results from an understanding of "what will something do?" and "what will be something's effects?" Underlying this is the necessity to understand "what is this something like now?" and "how did it get that way?" This is the realm of science, and its "purity" depends inversely upon how far it goes into the crass problem of "how can the effects, particularly on mankind, be altered?"

The sense of security that results from the description of something and from an understanding of the processes which explain its state and its probable future course must consist of a feeling of unity with the environment and must be what we speak of as "spiritual security." Is not the great potential benefit of science to mankind such that men might be relieved from the necessity of explaining physical events by supernatural forces? If so, it is the benefit which has no organized

group for its harvesting but which is left as an untouched reward for the worker and some interested gleaners.

The further sense of security results from having altered the effects. This is the province of engineering, which is charged with the imperative statement, "alter the effects of something!" The degree of the "applied" nature of engineering depends inversely upon the degree of its investigations into the questions, "what are the effects of something?" and "what is the something like?" Engineering goes far into these areas, especially where science does not see fit to go. And it does so because the rational approach to its problem of control must be founded on understanding.

Nevertheless, basically science is charged with the problem of "what will something do?" while engineering is charged with "do something about it." The sense of security gained from pure science is a security of the spirit and its reapings are poorly disseminated, while the sense of security gained from engineering is physical and its harvesting is highly organized.

These foregoing statements leave a great area untouched. Originally the "somethings" were natural[2] parts or processes of the universe, but today many of them have already been acted upon by man for their alteration. The sullying effects of man, except for those of the scientists involved, must not exist in the "something" under investigation if science is to be the purest.

This curious idea of the uncleanliness of man's effects on nature, or rather, of man's unnaturalness, has resulted in a great group of studies, intermediate between science and engineering, which science calls applied and which engineering calls basic. These studies are fostered more by engineering than by science, but they suffer from the dichotomy. Surely they are as true a study into the truth as any. Consider metallurgy. A "pure" metallurgy could inquire only into the nature of "native" copper, gold or meteoritic iron. Is there not as much truth in steel as in meteoritic iron insofar as the state of matter is concerned? Yet the metallurgy of steel is relegated to the applied category. Or consider the genetics of *Drosophila* as opposed to horse breeding.

I believe that this attitude and the resultant dichotomy would be entirely defensible had science pursued and organized itself for its most important task to man, the generation of spiritual security through understanding. I cannot see that this has been much considered. One may argue, of course, that the revelation of truth will, in fact, not result in spiritual security. To this speculation there is no answer, for its conditions have not been discovered.

[2] For later emphasis I leave this word as I first unconsciously used it.

For completeness I must continue the argument around the last radian of its circle.

Concerning some geometric theorem, one of Euclid's students asked, "What is its use?" whereupon Euclid directed his assistant, "Give the young man a coin, he needs must be paid for his learning." Thus Euclid stated the precept that "usefullness" from inquiry into truth was only of economic value. I believe that this may have been a far reaching theorem and that its corollary implies the benefits of pure science to the spirit.

It is increasingly apparent that no strict or even qualified definition can differentiate physical from spiritual security. In the light of Greek history or of the present state of mankind, spiritual security cannot exist without physical security. Thus the gambit, offered by Euclid, is again challenged. I believe there is no defensible statement in "purity" of science or in the use of the words "applied" or "practical" or "artifactual" or "military" or "natural" or "unnatural" as applied to any basic difference of investigations into truth.

Let us not associate the matters which are called "directed" and "free" inquiry with the commonly used terms "applied" or "pure" research. This association is frequently met. I think that there is no veracity in believing that any un-directed inquiry exists. All workers attack problems that are directed by some compulsion. I believe that the only type of *directing* of research which is highly objectionable is that which insists on answers before understanding can be attained. On the other hand, certainly answers can be requested when understanding which is probably adequate *has* been attained. This understanding can be attained only by investigation on a broad front, for the threads of truth are pervasive and interweave in a fashion that can be understood only in retrospect.

Let me quote from the Fifth Annual Report to the American Cancer Society by the Committee on Growth:

> As scientific advisor to the American Cancer Society, it is the responsibility of the Committee on Growth to formulate and implement a comprehensive cancer research program. At first glance, the problem of what is and what is not cancer research, may appear to be a relatively simple one. It is true that a number of competent scientists have devoted themselves to the study of cancer and laboratories and institutions have been established for the sole purpose of investigating the cancer problem. Many of the most important discoveries related to cancer, such as x-rays and radium, however, were made by people who had no direct interest in cancer. There is no simple way to determine with any degree of certainty whether a particular research project will make important contributions to our knowledge of the nature and control of cancer.

In 1941, to illustrate, Dr. Charles Huggins of the University of Chicago announced his extremely important discovery regarding the treatment of cancer of the prostate in man by administration of female sex hormones. Although the treatment does not constitute a cure, in a matter of days or a few weeks after treatment is begun, all of the symptoms are gone in many instances and the patient feels completely well. The treatment may be effective for a period of several years.

The foundation for Dr. Huggins' discovery was laid in 1889 by Dr. Joseph Griffiths of Cambridge University in England. Dr. Griffiths, studying the seasonal changes in the size of the prostate gland in the hedgehog, noticed the decrease in the size of the prostate gland when an animal was castrated; this indicated a relation of the male sex hormone to the gland. In 1912, the basis for the second aspect of the problem was formed when Drs. Grosser and Husler, working at a children's clinic in Frankfurt, Germany, first discovered the enzyme, phosphatase, in the membrane lining the intestine. Drs. Steinach and Kun in 1926 demonstrated that the injection of female sex hormones into male animals caused a rapid decrease in the size of the prostate gland. In 1934, Dr. D. R. Davis in Wales and Drs. Baaman and Riedell in Stuttgart independently discovered that there were two different phosphatases, one active only in an acid medium and one only in an alkaline medium. A year later Drs. Kutcher and Wolbergs at the University of Heidelberg showed that the secretion from the human prostate gland was very rich in acid phosphatase. In 1938, Drs. A. B. and E. B. Gutman demonstrated that the blood of patients with cancer of the prostate often contained more than the normal amount of acid phosphatase. The basic information was now available to lead Dr. Huggins to undertake his highly successful studies on the treatment of cancer of the prostate with female sex hormones.

How many of the above investigators who made important fundamental contributions to this great advance in the treatment of cancer would have been supported by funds for cancer research had such funds been available? As a comment on the question, the following paragraph is quoted from the book Science and the Planned State, by Dr. John R. Baker of Oxford University in England:

> 'What central planner, interested in the cure of cancer, would have supported Griffiths in his studies of the seasonal cycle of the hedgehog, or Grosser and Husler in their biochemical work on the lining membrane of the intestine? How could anyone have connected phosphatase with cancer, when the existence of phosphatase was unknown? And while it was yet unknown, how could the men in charge of cancer funds know to whom to give the money for research? How lucky it is for sufferers from cancer of the prostate that Griffiths and Grosser and others were not doing cancer research!'

Certainly these men possess the humility to recognize their lack of understanding and the importance of a broad attack. How different from research "by the numbers"!

To victimize you further in your helpless position, but also to clarify my position to a degree adequate for your competent judgment, let me sketch a notion of the hierarchy of physical science and engi-

neering. Central in this hierarchy are the studies of the great physical provinces, namely oceanography, meteorology, astronomy, and geology, drawing from all of the applicable established disciplines and proceeding toward an understanding of the complex processes in the provinces and their interrelations. Engineering is the study which, through its apprentice industry, reaps the results of the hierarchy for the *physical* benefit of man. Thus, whether it be fisheries, marine architecture, agronomy, applied mathematics, textile testing (or if you like, surgery), I will call it engineering in this broad sense.

In this picture, an established discipline, such as chemistry or genetics, is concerned with the study of apparently consanguinous aspects of several physical provinces. It is believed that these aspects are encompassed by some family of rules by which the worker may be able to answer the question: what will something do? In reality, a discipline attempts to set up an analogue of the parts or processes in one of several ways, depending on the complexity and developmental stage of the study. In complex cases, including most phases of biology, the discipline is an attempt to organize words in such a way that they will set up in the consciousness a mental analogue of the part or process, that is, to describe a static taxonomy or dynamic behavior. In the less complex subjects, such as physics, chemistry, hydrodynamics, physical oceanography, etc., an attempt is made to establish symbols and associated rules of counting which will cause numbers to behave like the physical quantities under study. Also, physical analogues are devised on the basis of assumed similarity of the counting laws, or descriptions of the model, to the part or process. That is, the disciplines attempt to establish a neural, symbolic or physical analogue which will re-enact processes in the past with the greatest attainable similitude so that other related future processes can be predicted with the greatest possible reliability. The symbolic approach is most capable of handling a number of rather simple and similar quantities that behave in simple rigorous ways, whereas the descriptive approach does surprisingly well with inductive results derived from description of a large number of highly dissimilar quantities which behave and interact in equally rigorous but highly complex ways. The great biological laws of evolution and biogenetic recapitulation have been induced from masses of description, although the inexpressible uncertainties of our understanding of biological processes are immeasurable compared with those expressed uncertainties of the uncertainty principle of atomic physics.

Frequently, in different disciplines the symbolic and descriptive approaches almost become fetishes. Both extremes certainly are suspect: the group which is content that the gyrations of its symbols

truly explain events without the necessity of observation and visualization as well as the group which believes that a description can be improved only by making it more detailed, each without understanding the processes in both quantitative and descriptive realistic terms.

There are many present evidences of relaxation; the rigorous symbolists content themselves with statistical approaches while the documentists also utilize these possibilities. Analogue computors, including models, are increasingly important. Yet there remains a great middle ground which contains a large number of problems in oceanography and engineering whereto there seems to be no adequate approach. These are problems wherein the processes, materials, and interactions are so complex and uncertain as to permit no rigor and too difficult of observation to permit adequate description; *yet quantitative answers are required.* It is not that engineer and oceanographer are reactionary in their approaches—on the contrary, they have employed the best discoverable attack on their many problems but have been stultified to a degree by those groups that have adhered strictly either to symbols or descriptions without searching the middle ground. I believe that here we have a large and neglected area wherein engineering and oceanography together can benefit.

Let me quote a simple example. For many years the refraction of ocean waves was well known. The engineer encountered wave damage to his structures from wave forces that produced failure or erosion. The general descriptive understanding of the refraction process was well known. The fisherman's principle of "the points draw the waves" was adequate for many needs but it was inadequate for the location and design of structures on a headland. At the same time, rigorous expressions for wave refraction existed so that the "distribution of wave energy on a ridge with parabolic contours and exponential contour spacing" could be handled. Between these two extremes were the real world conditions where real waves peaked up and damaged installations on a point that looked like no point on a parabolic ridge yet devised. The simple solution to this problem resulted from the rational use of all that was known of wave refraction. From the description of offshore waves, laborious step by step construction was started, each step being determined by the quantitative laws and modified by the descriptive understanding. Realistic waves were brought in over realistic bottom topography to a realistic shoreline by a process in which a draftsman was taught to move a pencil as a wave moves. Many vexing questions were resolved in this process; for example, the effect of small bottom features was no longer open to argument since it could now be visualized. I am happy to note that this solution in no way froze the attack on wave refraction.

Aids which facilitated construction were devised for the draftsman yet room was left for the exercise of judgment. The documentists reported more observations on wave refraction and the symbolists set about explaining these with excellent results and with an advance of understanding.

This is a simple example. Surely better ones could be quoted. Yet, simple as it is, I believe the implications are important. I am impressed by the dichotomy of approach in many important cases. Let us go beyond the last example to the virtually unsolved problem of the impulsive loading of complex structures. I believe it probable that such impulsive loading of ships, buildings, automobiles and human bodies is rapidly becoming man's most import environmental condition.

Let us say a pier is destroyed by waves, or a ship by explosions. When a pier is destroyed, the engineer determines the probable height of the waves, describes the nature of the original structure and the nature of the damage, perhaps makes some static calculations on the strength of the installation, and proposes an increase in the factor of safety of some of its members from 3 to 5. On the other hand, the symbolist writes the equation for the impulse of a sinusoidal or trochoidal wave, approximates the characteristics of the pier as a cantilever, and then permits the two to interact according to certain rules governing harmonic motion. It is little wonder that opinions differ so widely. Actually the process is far too complex to be handled by either alone, whereas together the workers are capable of describing the events step by step, not losing sight of the realism of particles in either pier or water or of the drag on attached fouling organisms in contemplation of gyrating symbols; thus a visualization, tethered by quantitative calculations, could be carried through at the most fundamental level of process. What are the forces? What do the particles do? No doubt, in the case of the pier or ship, the symbolic approach could be enhanced by drawing from seismology and expressing transient waves (flexural, shear or torsional) which are generated and which move through the body in some idealized way. For these early purposes are these not artificialities? Is it not sufficient to know the forces and particle restraints and to trace through the process as laboriously as necessary in order to create a breakthrough in understanding the complex processes for future realistic, understanding methods and generalizations, as we have seen in the simple case of wave refraction?

To express this more clearly it would be necessary for me to quote more examples. I believe such an attack is applicable in oceanographic problems of mixing, evaporation, wind drag, productivity,

sedimentation; in fisheries problems of population dynamics and availability; in naval problems of mine countermeasures and explosion damage; and in a host of other problems now being attacked from two aspects to the neglect of the middle ground.

I am sure that many persons will believe this to be a small point. Many are quite sure that present analytical or descriptive approaches are adequate. I will say that the approaches are slowly becoming so. Careful, thoughtful observations and rational "real world" approaches must sculp idealization. The consideration of fronts, ripples, bursting bubbles, white caps, surface slicks and spray as real conditions of the ocean surface will reveal the manner in which fluxes of matter and energy occur through the ocean-atmosphere interface in a way that never could be realized by the strict analytical approach. The scientists who have initiated this realistic work are to be congratulated.

To emphasize the degree to which our thinking is often circumscribed by the fictions that we have created, I would like to offer the challenge of two deceptively simple problems that I have been perpetrating on friends lately. One is for the oceanographer. How does a group of water particles at a depth in the ocean accumulate the pressure commensurate with that depth? And one for the engineers. How does an immersed vertical oceanographic cable reflect at the surface the total buoyant effect of the water over its length?

I pose these for fun. However, sometime, if you have the opportunity, consider them, not with vectors or free body conventions but with a realistic rational picture of how the water is aware of that lying above and how the cable can accumulate its information at the surface. You will be amazed at how meaningless become the fictions and smooth conventions with which we often limit the veracity of our thinking.

It is, of course, presumptious of me to discuss this matter so incompletely in the presence of so many whose understanding far exceeds mine. However, I believe I owe no apologies for the things for which I sue:

First, an unreserved recognition of the common brotherhood of investigation into truth, that there is no stigma of the "applied," indeed, that there is no defensible difference between the "applied" and the "pure"—in my opinion, any stigmata attach to "pure" science for having in a measure failed of its promise.

Second, the recognition that "free" untrammeled research is the scientist's responsibility rather than his license and that such research is demanded by the necessity to understand the complex weave of the threads of truth, which admits no extensive preconception of what is or is not important in an inquiry.

Third, that no relation needs to exist between the degree of application and freedom; but that serious lack of freedom always obtains when "directing" insists on a narrow attack or proceeds without understanding.

Fourth, a recognition of a great common ground of real world *problems amenable to unreserved rational attack by those of all groups who possess part of the truth and who themselves are morally responsible for their solution.*

Fifth, a free exchange of methods; tolerance and understanding of different approaches when brought to focus on an important problem.

In conclusion, I realize that this new structure and this community tell the tale better than I. For here exemplified is a community, a common brotherhood of scientists and engineers, bringing their energies to bear on the "somethings," many man-made, that we must understand and control for the security of mankind.

APPLICATIONS OF OCEANOGRAPHY TO NAVIGATION

By

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Navigators, concerned with the task of maintaining safe and efficient sea communications and faced with everyday difficulties of wind and weather, have to maintain a more practical outlook than oceanographers, who want to understand everything that goes on in the oceans however long they are about it. Oceanographers, being continually under the spell of the spectacular advances in scientific methods and techniques made in laboratories on shore, are keen on trying new methods. Nevertheless there is collaboration: navigators are not slow to adopt the results of academic research, and oceanographers are not slow to appreciate the value of established practices and the need for some caution in introducing new methods. Progress is made by what seems to be a fairly healthy mixture of personal enthusiasm, pressure from government departments, commercial enterprise, and guidance of professional associations; but strong government support is as essential to progress in navigation and oceanography as it is in several other branches of science which require facilities that are beyond the ordinary reach of Universities and that have not yet been provided by industry.

Although the chief justification for continued public support of oceanographic research must be the results and by-products of the work, the long-term nature of the necessary studies is sufficient to justify those engaged in them in relying to some extent on a plain statement of what they are about. The opening of a new laboratory is a suitable time for such a move, and it is my privilege to speak about the promise of oceanographic research for navigation.

Tides. The excellence of tidal predictions for ports of any size is sufficient to raise doubts as to whether there is justification for further theoretical research which is necessarily of a complicated nature. However, there are plenty of projects involving salvage, construction, beach landing, and other tasks which require accurate tidal information for places where no tidal records and analyses have been made and which may be far removed from the nearest standard port. Such demands are likely to call for someone with an active understanding

of tidal theory who is able to calculate the probable direction and changes in the tidal wave and to insure that such measurements as can be made on the spot are those which will give the most information. Moreover, there is little doubt that our knowledge of tidal streams is not as satisfactory as that of the times and levels of high and low water, hence much theoretical and practical research is needed to improve our understanding of them, particularly of the tidal streams in estuaries and channels. However, probably the most outstanding need is for detailed investigation of surges and other movements of water due to meteorological factors which disturb the normal rise and fall of tides.

Currents. The whole problem of the effect of wind on the sea is still in a most unsatisfactory state. We have reliable charts of average directions and speeds of surface currents and we know that they are caused by the effect of local and distant winds, but the charts often emphasize the variability of the currents. Too little is known about the rate at which wind drifts grow and decay to allow an estimate of the variation of movement in particular weather conditions which may make the actual set very different from the monthly average. Active research aimed at a better understanding of wind drifts is being carried on in most marine laboratories, but the subject is more difficult than the study of tides, since the forces involved, particularly the drag of the wind and the effect of friction and viscosity on the water, are themselves complex and scarcely understood. The new radio aids to navigation are making it much easier to fix the position of a ship, but they are not yet universal and undoubtedly it would be useful to know how the current is likely to change with varying wind and weather conditions. For example, it would be a good thing if the task of estimating the drift of a raft or wreckage at sea could be made easier and more precise.

Waves. Although ships manage very well in all weather conditions, storm waves are still one of their worst enemies, and it is reasonable to maintain that something must be gained from a better understanding of ship movement in relation to complex wave patterns. The master of a ship learns by experience how the pitching and labouring of his vessel varies with her speed in relation to the heights and lengths of the waves, but he would be the first to admit that there is plenty of scope for more information about the growth and decay of waves and about their effect on ship motion. This is particularly true for aircraft carriers in which the angle of pitch is of great importance. It is only during the past 15 years that realistic studies have been made of complex wave patterns, and, although we have made re-

markable discoveries about the distribution of energy over a range of wavelengths and about the rate at which the energy travels away from a storm, this is only a beginning. The detailed information that is being obtained about waves should be useful to ship designers; they have to allow a good margin of safety, but it would be surprising if more precise information did not lead to some improvements in design.

In addition to ordinary waves and swell there are several types of long waves which are of some importance to navigation. In certain harbours they may be a contributory cause to resonance effects, usually known as "range action," in which ships moored at a quay respond so actively to a backward and forward oscillation of the water that the mooring ropes are snapped and the ships damaged. The energy for the oscillation in the harbours is derived from several sources: it may come from surf beats (long waves caused by the alternation of groups of high and low swell) or from long waves transmitted directly from a distant storm; also, it may be derived from long-period oscillations of water level in the neighbouring coastal region relative to onshore or offshore water movements or from the partial deflection of the tidal stream. A detailed understanding of the sources of energy and of the processes by which such energy is transferred to the harbour could not fail to be useful in planning the position and design of new harbours, and it might facilitate improvements.

The study of processes which cause coastal erosion and changes in offshore bars and harbour approaches and which in turn affect landing operations on a beach might be regarded as belonging to the field of hydraulic engineering, but oceanographers who already deal with waves and sources of energy and who are interested in the interaction between water movements and topographical details on the sea bed can do much to help. Collaboration between oceanographers and engineers has already done much to improve our understanding of the processes which change the waves as they approach the coast. If the underwater contours and tidal streams are known, then variations in the height of surf from point to point along a coast can be predicted with reasonable accuracy. There is no doubt that the methods of wave prediction have reached a stage when, with the necessary meteorological information and some local knowledge, predictions of waves and swell will allow better timing in the transfer of cargo in an exposed anchorage, the passage of vessels across a bar, landing operations, salvage or construction. Further studies will undoubtedly aid coastal and harbour engineers in determining the best measures they can take to retain sand and

shingle or prevent it from being deposited where it is likely to become a hindrance.

Further examples can be found of ways in which studies of sea conditions, at present more or less academic, might find profitable applications in the future. One outstanding problem is the interchange of energy between the atmosphere and the oceans; as well as being essential to our understanding of water movements in the oceans, it is important to long-range weather forecasting, and meteorologists and oceanographers are collaborating in its study.

It is reasonable to maintain that the great amount of effort spent in using the sea and in defending our lives and possessions against it demands as detailed an understanding of its behaviour as may be gained by application of ever-improving scientific methods to supplement experience and simple interpretation of observations.

In conclusion I might mention that I prepared my talk a month ago in a small ship on the Atlantic Ocean when the weather was too bad for oceanography and when careful navigation was the order of the day. But with this caution, and remembering also that it is difficult to urge new work without the risk of undervaluing what has been done already, I feel no hesitation in urging new studies, however long they may take. I believe that most oceanographers will agree with a plea that the problems should be approached in a systematic long-term manner rather than by too direct attack. At the end of a direct attack it is often found that the target has moved so that only a wider spread of observations could have found the mark. However, the spread must not be too wide to get the range. It must be fairly general experience in oceanography, as in other sciences, that an academic approach often produces the knowledge, method or apparatus required for a particular application just when it is most needed.

THE IMPACT OF OCEANOGRAPHY ON OTHER SCIENCES[1]

By

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"Unity is perhaps the keynote of modern science. This means unity in time, for the present is but the outgrowth of the past, and the future of the present. It means of process, for there seems to be no sharp dividing line between organic and inorganic, physical and mental, mental and spiritual. And the unity of modern science means also a growing tendency towards cooperation, so that by working together scientists discover much that would else have remained hid."[2]

The title of my paper is one that was proposed by the committee that planned the Oceanographic Convocation. The subject was a challenging one, but when I attempted to set down ideas it became obvious that the problem was not so much one of pointing out interesting problems that had stimulated research in the basic or related sciences but rather one of determining the unique features of oceanography as a whole and how these might be of importance in the growth of human knowledge. Some aspects of this problem, described in a recent paper,[3] will not be dealt with in any detail.

For the purposes of this discussion, it is necessary to establish certain terms of reference. The science of oceanography is still in a formative stage; subject matter is broadening as new topics are investigated, and it is obviously pointless to define the field in any restrictive way. Therefore, a broad definition, that *oceanography is the scientific study of the oceans and of their relation to man*, is the one that will be used. There can be little argument about the first phrase of the definition, but a word of explanation about *their relation*

[1] Contribution No. 196 from The Department of Oceanography of The University of Washington.

[2] Preface to *Climatic Changes: Their Nature and Causes* by Ellsworth Huntington and S. S. Visher, Yale University Press, New Haven, 1922. 329 pp.

[3] Fleming, R. H., 1955, Present status and future development of oceanography. Proc. 8th Pacif. Sci. Congr. (Held in 1953.)

to man may be in order. This too is intentionally broad. The most obvious aspects covered by this phrase are the practical or engineering applications of knowledge of the seas, how the design and performance of vessels are affected by the environment in which they must operate, or the nature and size of fisheries that might be developed in the unexploited waters of the southern hemisphere. The oceans have exerted, and continue to exert, major effects on the cultural, economic, and political histories of mankind. These are not generally included in the field of oceanography, but the *scientific* aspects of these problems offer many opportunities for co-operative investigations.

It is not necessary to stress the tremendous size of the oceans—that requires such great expenditure of effort for their systematic study—nor the complexity of the problems that involve the specialized techniques and theories developed in practically every other field of science. But it is important, in the face of these obstacles, that we do not lose sight of the concept of unity so well expressed in the quotation from Huntington and Visher. It is the unity of the oceans that can give to oceanography a unique character among the sciences.

Oceanography is not circumscribed by the traditional boundaries of the older sciences; it is truly world-wide, recognizing no political boundaries; and finally it is concerned with the environment that contains the uninterrupted and coherent physical and biological history of the earth. That is, the unity of the oceans can be recognized in subject matter, in space, and in time. The ultimate value of oceanography among the sciences will be found in the influence it exerts by bringing together, in a coherent whole, the theories and knowledge of the many specialists and by stimulating the scientific curiosity of those in the basic and related fields of science by presenting to them those problems that require their special talents. This is a period of specialization in science, and oceanography, to preserve its unique character, should strive to continue to be a co-ordinating study. It is only through such a broad attack on the oceans that their true nature will be understood and the full value of oceanography achieved.

The facilities available to oceanographers determine, to a large extent, the type of studies they will undertake. Some, with large vessels at their disposal and with adequate staffs will explore the far reaches and great depths of the open ocean. Others, with less resources, will limit their efforts to shallow and confined coastal waters. Governmental agencies, such as hydrographic services, may be concerned with only one or a few aspects of the ocean, but their interests may be world-wide in scope. The important point is that all of them are dealing with a single body of water and that all of their

contributions serve to swell the body of knowledge. If any single set of objectives can be agreed upon, these will influence not only the lines of research effort but will accelerate the rate of development of the science of the seas.

It is believed that the objectives of oceanography are *to understand the present, to interpret the past, and to predict the future.* Understanding the oceans of today is in itself a tremendous task, and so far we have sketched only an incomplete and fragmentary picture. In many cases, the key to understanding may be in the past. One of the basic principles of geology is that the present is the key to the past. This applies to the ocean as well as to the land (as will be explained later), but for the moment the concept will be inverted—that is to say, the key to present-day conditions in the ocean may very well be in the past. Recent interest in the origin and age of the deep water of the North Atlantic is an example.[4] It has been suggested that the formation of deep water in the North Atlantic occurs only during periods with extremely cold winters and that the water now present in the deep basins was formed during a period of climatic extremes, about 150 years ago. If this hypothesis is correct, it is obvious that the origin of the existing deep water cannot be explained in terms of present-day climates and that the key to the problem lies in climatic and oceanographic conditions that existed in past centuries.

A more generalized situation, which covers a span of many thousands of years, is the oceanography of the inland waters of the State of Washington, including Puget Sound and the waters adjacent to the San Juan Islands. The physical, chemical, biological and geological oceanography of this region cannot be understood without taking into account the geological history of the area. The extensive glaciers of the late Pleistocene carved deep, steep-walled valleys into the unconsolidated sediments deposited during periods of earlier glacial activity. It is the shapes of these features, now forming the beds of the channels, inlets and sounds, that largely determine the physical characteristics of the water as well as the nature and rate of water exchange in the partially isolated, elongated basins. The distribution of the plants and animals inhabiting the beaches and sea floor of this area will be understood only if it is recognized that some 10,000 to 15,000 years ago the entire area was filled with ice and that, since the retreat of the glaciers, the marine population has been re-established by migrants from the open ocean. The apparent mixture of "northern" and "southern" forms in the area probably indicates

[4] See in this issue: C. O'D. Iselin, Recent advances in our understanding of the circulation problem and their implications.

that the populations have not yet reached a climax. Certainly the challenge of establishing the chronology of the repopulation of the area and its correlation with the gradually changing physical environment is one of the principal problems that should be attacked by the oceanographers and biologists in the area. Finally, it should be pointed out that the submarine geology of the region is primarily one of working out the past history of the submerged areas. Processes of recent sedimentation are largely governed by the submarine topography produced by glacial action, and in many areas glacial debris is still exposed on the sea floor. Most investigations must inevitably lead towards an integrated geological, physical, biological and climatic history of the area. Other examples concerning larger regions and longer spans of time could be cited in support of the thesis that the key to the present lies in the past, but the important point to be recognized is that the history of the oceans is a continuous one and that "the present is but the outgrowth of the past."

The behavior of marine animals is receiving an ever increasing amount of attention. In many cases this involves study of the response of animals to the physical or chemical stimuli that they might encounter in the ocean; or it involves the development of hypotheses based on these environmental conditions to explain such behavior as spawning migrations, season or dates of spawning, diurnal vertical migrations, etc. In these investigations, emphasis is placed on the environmental conditions as they exist today. Distributions of organisms have been affected by past conditions in the ocean, and it seems equally probable that their behavior may depend, in part at least, upon the past environments as well as upon those existing today. The key to such perplexing problems as the migrations of fishes and the spawning habits of many invertebrates may very well be in *relict behaviors* that had survival value under conditions quite different from those in the ocean of today.

The concept of the continuity in time of all aspects of the oceans is equally important when we attempt to re-create conditions of the past. It is only in the present-day ocean that we have any opportunity to evaluate the interdependence of various physical factors and to develop the theories that are essential if we are ever going to properly reconstruct the oceans of the past, and, through such knowledge, to better interpret the history of our planet. Let us consider only a single example. It would be of great interest to estimate the abundance of life and the productivity of oceans of the past. To accomplish this we must have a quantitative understanding of the physical factors that control the supply of nutrients to the photosynthetic layer and the rate of turnover of such limiting substances

in the surface layers. To date our understanding of these processes is only fragmentary, but it is both useful and stimulating to examine a problem such as that of productivity, not only in terms of present conditions but also as to the types of information that must be at hand in order to attempt any valid estimate of the productivity during past geological ages under different environmental conditions.

Many of the problems in applied oceanography resolve themselves into predictions. This is a sweeping generalization that requires some explanation. A navigational chart is valuable because the shoreline and bathymetry generally change so slowly than an accurate survey is an excellent prediction of what a navigator will encounter when approaching an unfamiliar coast. The fact that charts and sailing directions must be corrected and revised at fairly frequent intervals is an indication that some significant changes do occur in nature as well as in knowledge. Other phenomena change in a regular and predictable way. The practical value of tidal information and justification for the efforts devoted to observing and compiling such data and issuing predictions lies in the fact that these are reliable forecasts of the stand of sea level and are a major factor in safe navigation in shoal waters. Tidal predictions can be made for any date, past or present, but they are subject to errors introduced by local meteorological conditions. Storm surges are catastrophic examples of abnormally high sea levels caused by onshore gales or hurricanes. Such devastating inundations cannot be predicted long in advance; but because of their inherent threat to life and property, methods of predicting their effects on sea level have been devised for individual localities. Predictions of storm surges are based on the known meteorological situation. Hindcasting is a useful corollary of such methods, since it permits us to reconstruct past situations when the significant variables are known. The examples just cited illustrate cases where methods of predictions have been devised to meet practical demands.

There are, however, many more instances in which no satisfactory methods have yet been devised. No techniques have been developed that will enable us to predict future temperature conditions, current patterns, the distribution and abundance of fish, or the fate of radioactive isotopes introduced into the sea, to name but a few. In most instances the serious gap is our ignorance of the processes relating cause and effect. Qualitative forecasts based on past experience, that is the use of *climatic* data, are useful in some cases, but they lack the local detail and accuracy that is desired. Similarly, empirical relationships derived from observed events are often used, but without a basis in theory. Such methods of predicting can be grossly misleading.

The foregoing paragraphs have emphasized the prediction of future events or conditions. There is another whole realm of practical problems that involve predictions of the influence of oceanographic factors upon the behavior or effectiveness of vessels, shore installations, offshore platforms, underwater weapons, and devices for communication and detection. These estimates require not only an understanding of the ocean conditions but an appreciation of how these will interact with the object of concern. Although some of these examples of applied oceanography may stretch the definition of the term *prediction*, they serve to emphasize that it is upon a firm foundation of basic investigations and research that the practical applications of the future must be built.

The unity of the oceans in space is one of the basic principles of oceanography. It is the basis of the physics and chemistry of the sea where it is accepted that the properties of the water depend only upon its temperature, salinity and pressure and not upon the location of its existence in the sea. Effects of dilution by rivers, of freezing and melting sea ice, and of organisms are recognized, but they are treated as anomalies rather than as major regional influences. The age of the oceans, the magnitude of the disturbing factors, and the processes of mixing that produce the characteristic uniformity of ocean waters apply equally well to biological and geological aspects of the sea. The interesting problems are not differences in populations and sediment types as they exist but rather *why* they exist and *how* they are perpetuated. The principles of unity in space, unity in time and unity in subject matter are the factors which will weld the subject matter of oceanography into a coherent science.

The concepts outlined above are more than mere philosophical ramblings. They can provide guidance in teaching, in research, and in reporting results of field work. Specialized research and local studies are of much greater interest if they are set in the framework of broader generalizations. Artificial subdivisions of science are to be avoided if studies in the physical, chemical, biological, or geological aspects of the sea are to be presented as parts of the entire picture. Futhermore, it is through such a unified and coherent science that oceanography can make its greatest contributions to human knowledge and can have its greatest impact on other sciences.

In the terms used in this paper it cannot be said that oceanography as a science has had much effect on progress in other fields of research. On the other hand, individual oceanographers who, in their work and writings, have stressed one or more of these concepts have had very large effects on their fellow workers in other fields. The work of Matthew F. Maury a century ago is often cited as the beginning of

oceanography. He was very aware of the unity of the oceans in all their aspects, and it was with this guiding principle constantly before him that he was able to stimulate so much interest in marine problems. Others, perhaps placing more emphasis on the biological characteristics of the oceans, prefer to date the birth of oceanography from the *Challenger* Expedition. It was the broad interests of Sir Wyville Thomson and Sir John Murray that made this venture so important and so productive. In this age of specialization it would be most regrettable to lose that which makes oceanography unique and which had such an auspicious start under men of broad vision. The problems of the ocean are among the most difficult that must be solved. Their ultimate solution will depend upon the co-operation of scientists in many lands who must draw upon the theoretical and technical advances in many fields of science. It is the responsibility of oceanographers to develop a unified body of knowledge and to stimulate the interest of those in the basic sciences and related fields of natural science so that they may "discover much that would else have remained hid."

~ *BIOLOGICAL OCEANOGRAPHY*

DEEP WATER MOVEMENTS IN THE NORTH ATLANTIC AS A LINK BETWEEN CLIMATIC CHANGES AROUND ICELAND AND BIOLOGICAL PRODUCTIVITY OF THE ENGLISH CHANNEL AND CELTIC SEA[1]

By

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Since World War II, a major investigation has been pursued at Plymouth to establish the cause of the great changes which have taken place in the waters of the English Channel in the last 30 years. About 1930 a productive water, relatively rich in nutrients and macroplankton, was replaced by a poorer one. We sought the cause in the neighbouring shelf waters and found none. Whatever the cause, it was transmitted through the Atlantic Ocean. Today a level of nutrients similar to that found in the Channel in the 1920's occurs only at 700 m, from which no known mechanism can cause it to upwell.

An hypothesis was erected that 30 years ago the intermediate water had been upwardly displaced by at least 400 m and that this had come about by intrusion of deep cold water from polar regions on a scale much greater than that now taking place. It seems that the additional cold deep water came mostly from the north rather than from the Antarctic and that the only place in the north where it could have been formed was around Iceland and the Faeroe Islands.

Three water masses mix to create the water over the Faeroe-Iceland Ridge: the Norwegian Sea deep water with a salinity close to

[1] Between delivery of this paper at Wood's Hole and printing I intended to add factual evidence and authority for many statements which may appear unsupported and controversial. However, to avoid delay in publication, the Editor and I have agreed to send forward the script as it stands. Detailed argument and acknowledgments will appear in a series of papers yet to be written. Indebtedness to many Scandinavian, American, and British colleagues should be clear; that to the German workers may not be so evident. The essential synthesis with their work first requires an assessment of all available deep silicate analyses which has not yet been done.

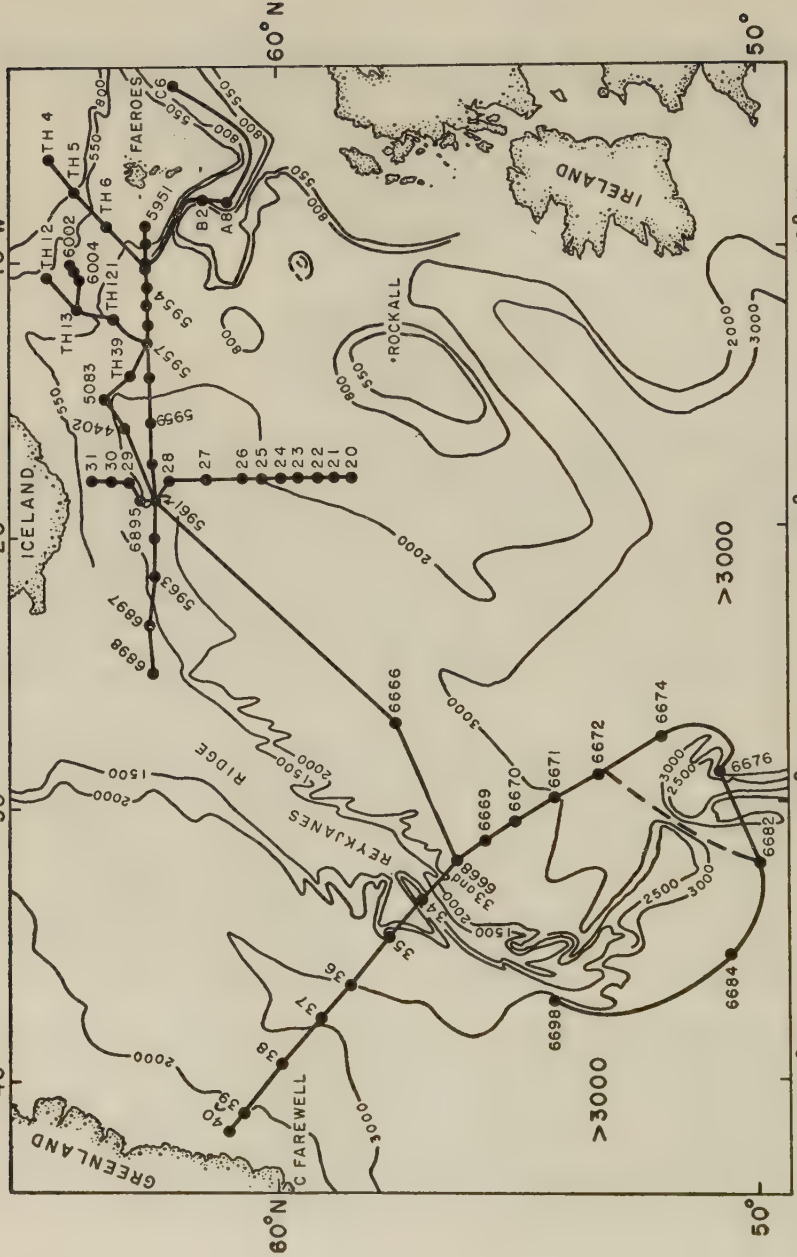

Figure 1. Positions of stations and sections in the northern North Atlantic (see Sources of Information).

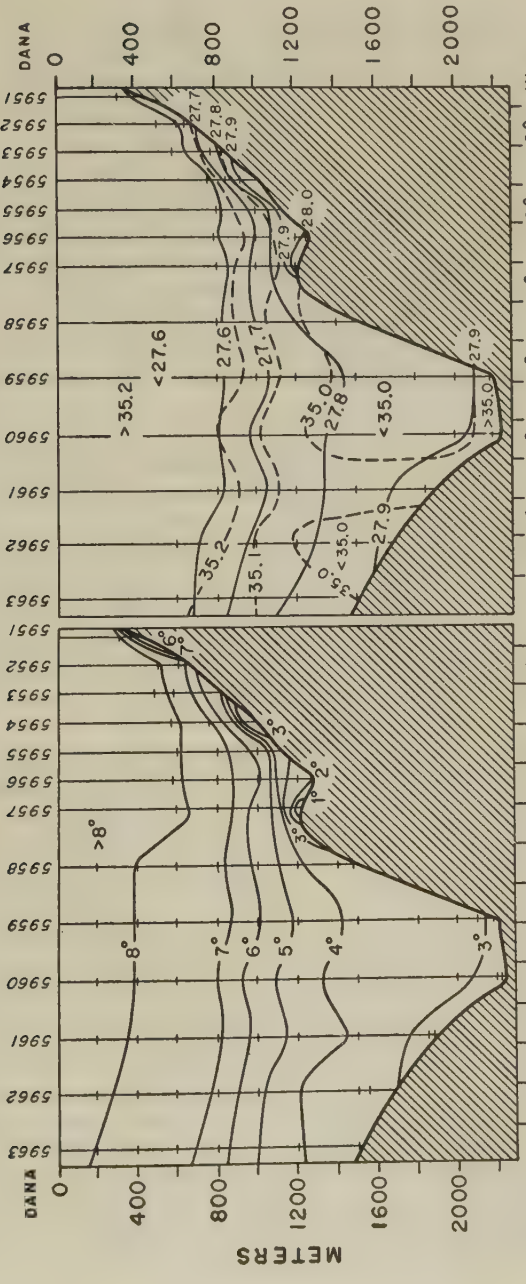

Figure 2. Section along the parallel of Lat. 62° N on the southern flank of the Faeroe-Iceland Ridge. Tributaries or, more probably, boluses of water are seen at Sts. 5954 (61° 59′ N, 10° 54′ W) below 870 m and 5957 (61° 58′ N, 13° 04′ W) below 1150 m (Legend amended 6.xii.54)

34.92‰ and temperatures below zero; the cold East Iceland surface water with salinities around 34.8–35.0‰; and the warm Atlantic water with salinity over 35.2‰. There is no difficulty in finding mixtures of these waters which give a water with the properties that we find in the Faeroe-Iceland water.

Fig. 1 shows the northern North Atlantic. There are two main deep channels leading to the Denmark Strait and the Faeroe-Iceland Ridge. Between lies the Reykjanes Ridge with a long extension at depths between 1000 and 1500 m south to Lat. 52° N. The sudden deepening here will be called the snout of the Reykjanes Ridge. The Faeroes lie on a ridge extending from Iceland to Shetland. Cutting through this between the Faeroe Islands and Faeroe Bank is a narrow pass about 850 m deep. For today I am calling this the Syderö Channel.[2]

A homogeneous section was worked by the DANA at the beginning of July 1938, along 62° N from the Faeroes to the Reykjanes Ridge (see Fig. 2). You will see that small streams of cold heavy water with a salinity greater than 35.0‰ apparently cross the section at Sts. 5954 and 5957. These cold heavy currents may seem trifling but they are not. You will notice that, farther on at Sts. 5959–5962, this heavy cold water reappears, banked against the channel's western boundary on the Reykjanes Ridge. You will note also that there is an increase in salinity at the bottom and that the bottom water is similar to that 1000 m higher at St. 5954. St. 5961 is the nodal point of many of the sections to be discussed.

The heavy waters at Sts. 5954 and 5957 can be traced back onto the Faeroe-Iceland Ridge (Figs. 1, 3, 5). You will see that isotherms, isohalines and isopycnals slope steeply over the Ridge from north to south. This does not mean, however, that heavy water need flow straight down them from Norwegian Sea to Atlantic Ocean. They represent conditions which must exist if a powerful steady current is flowing along the length of a ridge with smooth contours. Under such conditions the current acts as a dynamic dam that parts the waters of the Norwegian Sea from those of the Atlantic. We know from the work of Nansen and Helland-Hansen that the current may be strong, but we also know from charts that the bottom is

[2] This name did not receive the approval of the British National Committee on Nomenclature of Ocean Bottom Features. That Committee, meeting on 30 June 1954, considered the proposed name "Syderö Channel." It was of the opinion that Syderö is a common name of many Scandinavian islands, and it suggested a more appropriate name, "South-western Faeroe Channel" (Royal Society memoranda NOB/9[54] and NOB/11[54]). In any quotation from this paper, the name "Syderö Channel" should be replaced by "South-western Faeroe Channel."

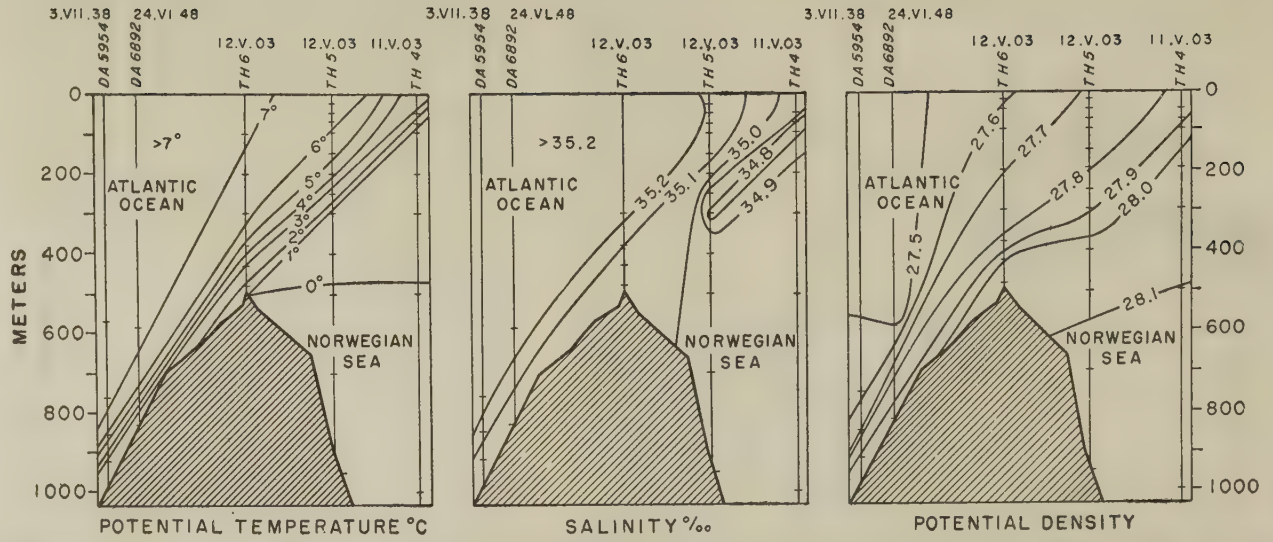

Figure 3. Composite figure to illustrate (1) the dynamic dam parting the waters of the Atlantic Ocean from the Norwegian Sea over the Faeroe-Iceland Ridge and (2) the tendency of water having potential temperatures between 1° and 4°, salinities exceeding 35.00°/oo, and potential densities exceeding σ_t 27.9 to cascade over the Atlantic flank of the Ridge.

Positions and dates:

TH 4	11 May 1903	63° 56′ N	6° 20′ W
TH 5	12 May 1903	63° 10′ N	7° 31′ W
TH 6	12 May 1903	62° 40′ N	8° 44′ W
DA 6892	24 June 1948	62° 00′ N	10° 20′ W
DA 5954	3 July 1938	61° 59′ N	10° 54′ W

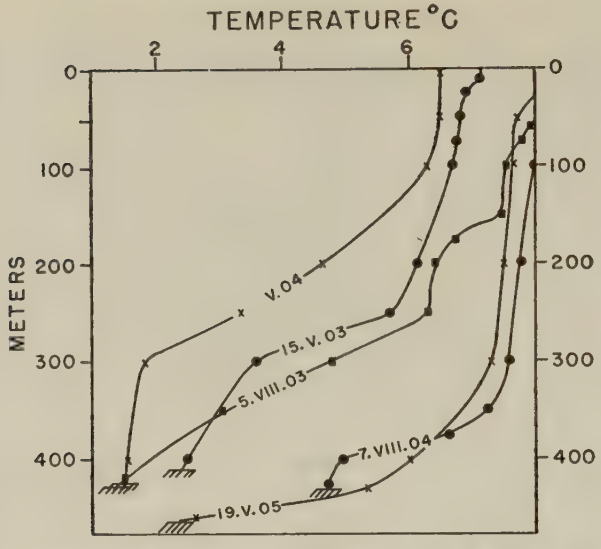

Figure 4. Variations in temperature on five occasions at a single place (Thor St. 13 at 63° 12′ N, 11° 45′ W) just on the Atlantic flank of the Faeroe-Iceland Ridge. The figure suggests that the Iceland-Faeroe Current may meander as the Gulf Stream seems to do. The nature of the water below 300 m on 4 May 1904 (temperature 1.5–1.8° C, $\sigma_t > 27.94$) represent conditions that are believed to attend the birth of a bolus of cold heavy water. The note "Starke Abdrift" (= strong leeway or deep current?) was entered against the record for the 400 m sample on this occasion and against the record of depth (435 m). (Legend amended 6.xii.54).

neither smooth nor the contours regular. It is also unlikely that the current flows steadily year in, year out. If the current were to weaken, the isopycnals would flatten so that warm saline surface Atlantic water would flood farther into the Norwegian Sea whilst the deeper heavier water above sill level would cascade over the Ridge into the Atlantic to the south.

Again, to judge from the increasing knowledge of the Gulf Stream, powerful currents do not follow unchanging courses. They form eddies and swing about all over the place. If the Iceland-Faeroe Current does this—and I can see no reason why it should not—then heavy water masses will at times swing to the south over the Ridge towards deeper water, where they will become unsupported and will sink rapidly into the Atlantic.

Yet again, it would seem that canyons cut back into the Ridge. If the main east-west current passes over a canyon at right angles, heavy water will drop into it and be unable to continue forward with the current. It can follow only the course of the canyon down into

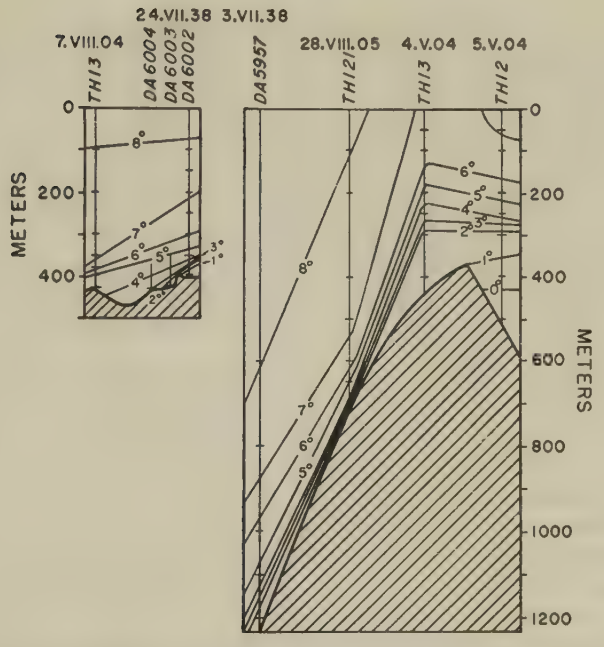

Figure 5. Composite pictures from the same area, the left-hand one showing conditions adverse for the formation of eastern Atlantic deep water, the right-hand one showing favourable conditions.

the Atlantic. The last process should yield a steady stream of water. Thus there may be three distinct mechanisms by which heavy cold water from the Faeroe-Iceland Ridge may slip into the Atlantic.

In Fig. 4 are shown temperatures at THOR St. 13, just on the Atlantic side of the crest of the Faeroe-Iceland Ridge. It was worked five times, three times in May and twice in August. The great variation in conditions suggests that hereabouts there is either a great variation in the strength and direction of the Iceland-Faeroe Current or that there are large temporary meanders.

Again in Fig. 5, THOR St. 13 is included in both drawings. The small drawing on the left represents conditions when no water is flowing over the Ridge and when the east-west current is weak. The right-hand drawing, very composite, illustrates conditions when cold ridge-water is pouring down into the Atlantic. The density diagram is much like this. There is no question that cold heavy water is entering the Atlantic over the Ridge, but on this evidence alone one could not say that it is happening on a scale that matters.

As water sinks in the sea it is warmed adiabatically by compression and in any study of vertical movements in the sea, this adiabatic effect needs to be eliminated. So observed temperatures have been converted to potential temperatures throughout.

No straight line section can illustrate my thesis. Fig. 6 shows a section along the axis of a river of heavy water that contours the sides of the Faeroe-Iceland and Reykjanes Ridges. From EXPLORER St. C6, well inside the Norwegian Sea, I have drawn the section so that it follows the approaches to the Syderö Channel, passes through the Channel into the Atlantic, then traverses the ridges to DANA St. 5962 (1938) and finally extends up onto the Ridge at DANA Sts. 6897 and 6898 (1948).

All observations were made around midsummer in different years. DANA St. 4402 would have fitted better if it had been 10 miles farther north; if it had been, the isopycnals might well have run smoothly from THOR St. 39 to DANA St. 5961, where Figs. 6 and 8 may be considered to make an end-on junction.

It is clear that the cold heavy water does not take a direct course to the bottom of the main channel but traverses or contours the sides of the deep inlet. The picture is like the Missouri-Mississippi Rivers system. The longest arm, EXPLORER St. C8 to DANA St. 5953, does not contribute the most water. That comes in from the Ridge near DANA St. 5957 at 62° N, 13° W. This tributary always seems to have a salinity exceeding 35.0‰, however cold it may be, and it is this tributary which causes the salinity inversion and subsequent increase in salinity downwards, as you will see in subsequent figures.

In June 1938 a most valuable section was worked by the Scottish vessel EXPLORER along the 18th meridian west (see Figs. 1 and 7). I have included in this section our "nodal" point, DANA St. 5961, which was worked a fortnight later, and DANA St. 6895, which was occupied in 1948. You will see how strongly the heavy cold water is banked, like a blanket, against the north side of the Channel. There is no doubt whatever where this water has come from; it is moving westwards with the heavier water on the right.

The intermittent bursting of the dynamic dam, due to either process, would result not in a continuous stream of water flowing off the Ridge but in a series of self-contained, cold heavy balls or boluses of water. The calving of a large bolus of water from the Ridge is likely to be completed in a matter of days or weeks at most. Its descent from 400 or 500 to 1500 or 2000 m through much lighter water is also likely to need only a short time. It is not surprising, therefore, that no research ship has ever recognised the birth process for what it may be.

We have now finished with the area around Iceland and will turn

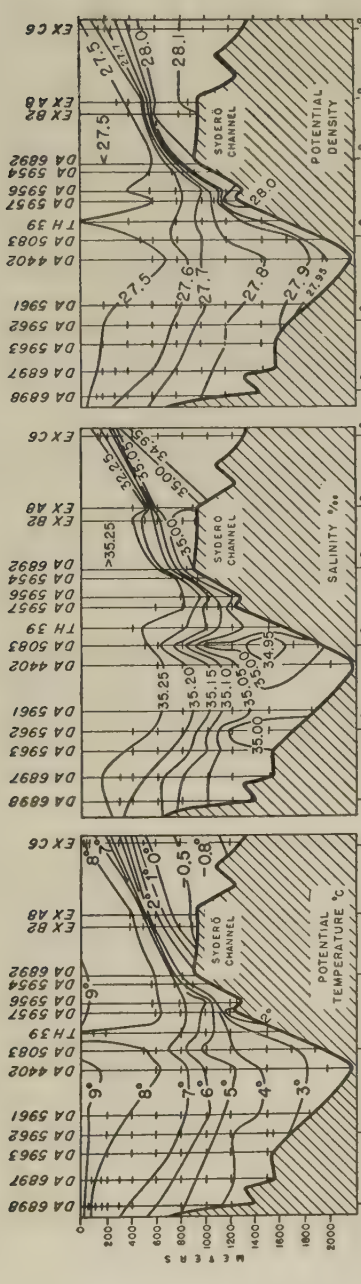

Figure 6. A composite picture constructed so far as possible from observations along the presumed course of the Faeroe-Iceland heavy water as it sinks whilst traversing the Atlantic flank of the Faeroe-Iceland Ridge.

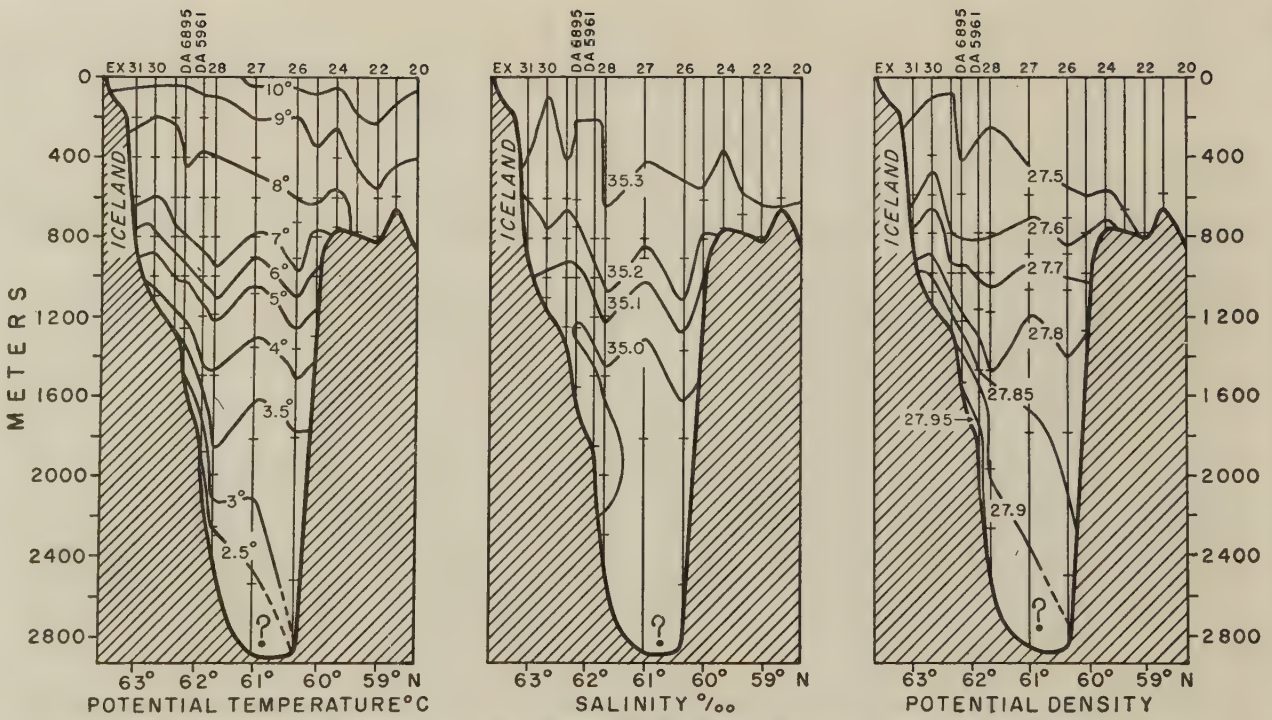

Figure 7. A section along the meridian 18° W, constructed from Explorer stations worked between 17 and 20 June 1938, from a Dana station made a fortnight later, and from another Dana station made on 25 June 1948. Note the blanketing of the Iceland insular slope by water colder than 3°, denser than σ_t 27.9 (both potential) and having salinity mostly between 34.97 and 35.02°/oo; also note the salinity inversion at Explorer St. 28. At Explorer St. 27 at 2540 m., potential temperature 2.46° and salinity 35.13°/oo were reported. The salinity, though unsupported and omitted from this illustration, is not wildly improbable.

our attention to the Reykjanes Ridge (see Figs. 1 and 8). Unfortunately there is a dearth of stations along the eastern edge of the Reykjanes Ridge, the only positions being those worked by the DANA in June 1947.

In Fig. 8 the depth to the bottom for each station is shown separately. The bottom contour represents the estimated depth of the deepest part of the main channel which lies abreast and to the east of the section as far south as DANA St. 6674. There is something odd about DANA St. 6668. Otherwise, as the main channel deepens, all isolines drop with it. In Fig. 7 we followed the Iceland-Faeroe Ridge water as far as St. 5961. In Fig. 8 we picked it up again at depths greater than 1500 m; potential temperatures between about 2.6° and 3.3° C: salinity greater than 34.97‰; and potential densities greater than σ_t 27.88. You will see clearly the increase in salinity towards the bottom as a result of water from around position 62° N, 13° W. You must picture the Reykjanes Ridge as shoaling to less than 1500 m to the northwest of the DANA section 5961–6674 and as impounding the Iceland-Faeroe water in the Eastern Basin.

Between DANA Sts. 6674 and 6682 the section turns westward and crosses the mid-Atlantic Ridge at the snout of the Reykjanes Ridge in 2700 m of water. Thence the section is in the Western Basin of the Atlantic.

All the curves show symmetry around St. 6682, which can only mean that the Coriolis force has caused some of the deep Iceland-Faeroe Ridge water to turn the snout of the Reykjanes Ridge and then to spread out toward the west and north. This is the water which Floyd Soule found in the Western Basin in 1935 and which he recognized as coming from the east because of its relatively high temperature.

I will now present some of these data in another form together with METEOR observations taken in August 1935 (see Figs. 1, 9).

METEOR St. 33 and DANA St. 6668 were two miles apart—at almost the same place. The dotted lines across the vertical bar, representing lapse of time at the same place, show that vertical oscillations of isopycnals and isotherms were as much as 300 m. To my mind these suggest either variations in the strength of the current in different years or the passing of boluses of water separately calved from the Faeroe-Iceland Ridge.

You will see that there is little difference in density between the bottom waters on the western flanks of the two basins, the higher temperature in the Eastern Basin being compensated by higher salinity. In the Greenland Sea there is evidently a cyclonic circulation of the 1500–2500 m layer, which contains Faeroe-Iceland water flowing

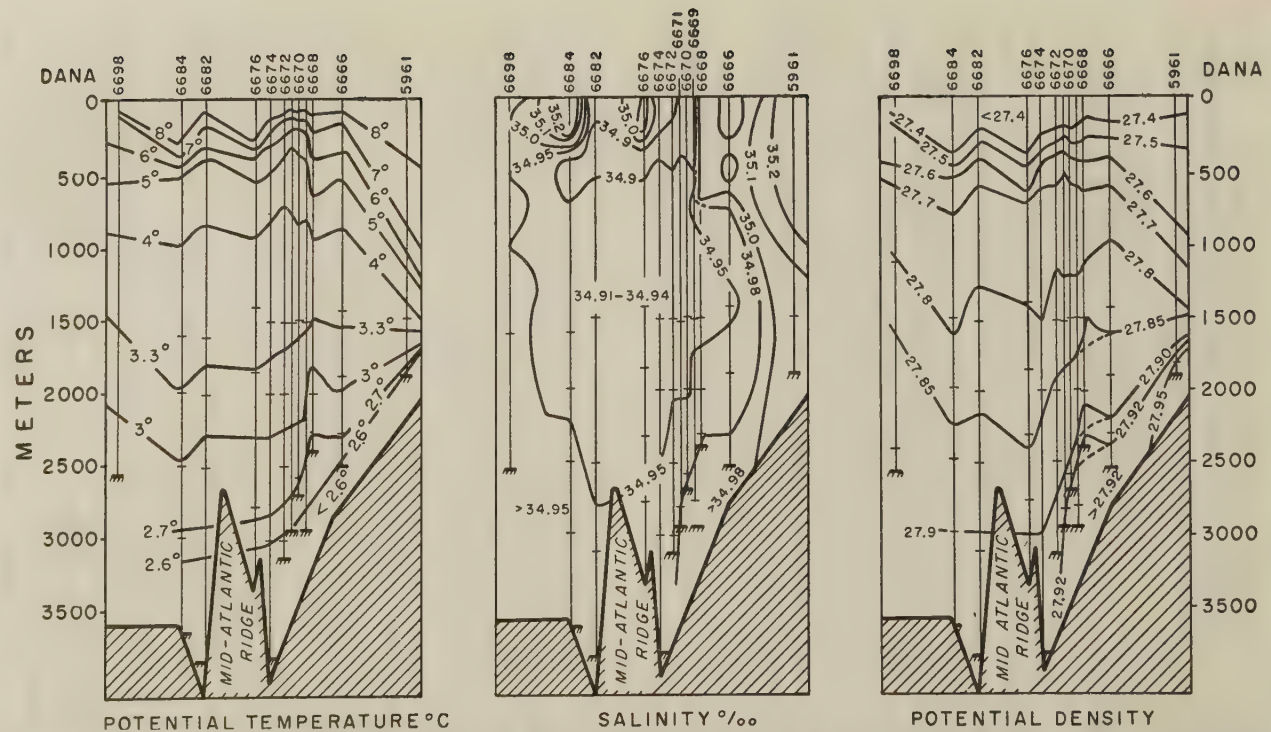

Figure 8. From Dana Sts. 5961 (4–5 July 1938) and 6666 to 6676 (17–21 June 1947) the section lies over the eastern flank of the Reykjanes Ridge. For each station, depths are shown separately. The greatest depth of the eastern North Atlantic Basin abreast of each station is shown by the firm continuous bottom contour. Except for St. 6668, isotherms and isopycnals fall parallel with the gradient of the bottom and indicate water sinking from the northeast. Stations on other occasions and by other ships show that this sinking is confined to the area over the eastern flank of the Ridge and does not occur on the European side of the Eastern Basin. At St. 6676 the section turns the snout of the Reykjanes Ridge so that Sts. 6682, 6684 and 6698 lie in the Western Basin.

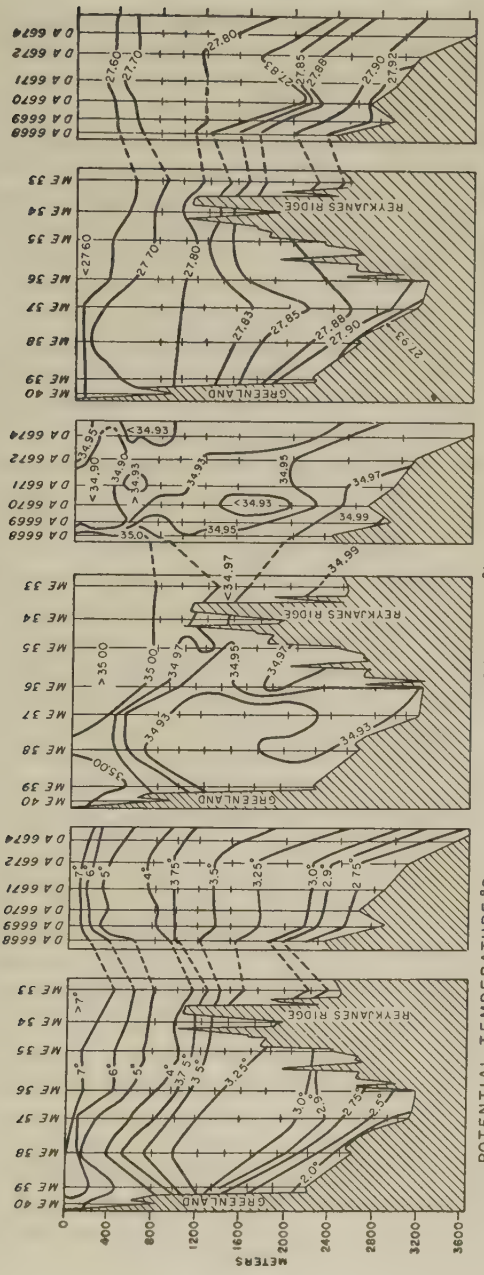

Figure 9. Potential temperature, salinity, and potential density for section from southeast to northwest, showing parallel behaviour over the eastern slopes of the Reykjanes Ridge and of Greenland. For the former it is suggested that the flow is from the Faeroe-Iceland Ridge, for the latter from the Denmark Strait. In the Western Basin between 1500 and 2500 m depth, a cyclonic pattern of flow is indicated. The pecked lines between Meteor St. 33 and Dana St. 6668 (almost the same place) show that, in time, oscillations of 300 m may occur. The salinity inversion at St. 36 is in line with the properties of Faeroe-Iceland water.

north on the eastern side and which, after augmentation by more very cold water, flows back along the western or Greenland side. Fig. 9 shows the salinity inversion on both sides of the Ridge.

To my mind there is no doubt that this considerable body of water with such characteristic properties, particularly the salinity inversion, can have only one origin. The evidence from the EXPLORER section along 18° W and from the DANA stations along the Reykjanes Ridge, supported by observations by the METEOR and the GENERAL GREENE (which I have not time to discuss) in the Greenland and Labrador seas, leave little doubt that we have here an event of much importance to an understanding of the circulation of the North Atlantic.

Note that the same distribution of properties at DANA Sts. 6668 to 6674, lying at an angle with the Ridge, has been used to buttress two different arguments; only one of them need be true. However, justification may be found in the Western Basin where enough observations exist to develop and resolve the two arguments independently.

There is evidence to suggest that intermittent calving, similar to that over the Faeroe-Iceland Ridge, occurs over the bottom in the Denmark Strait west of Iceland where the boundary between warm and cold water masses is sharp. One position is around Lat. 66° 20′ N, Long. 26–27° W, where calving is thought to be due to bursting of the dynamic dam between 200 m depth and the sill at 400 to 500 m. The evidence at the second position, around Lat. 66° 30′ N, Long. 30° 30′ W, rather suggests the subsidence of water travelling along the Ridge into the transverse submarine valley which runs north-south in Lat. 66° N, Long. 31° W toward Kangerdlugssuak.

The descending heavy water seems to be held to the right against Greenland just as that from the Faeroe-Iceland Ridge appears held against the Reykjanes Ridge. Sections which have been worked at right angles to the coast sometimes by chance cut through a cold bolus (cf. potential temperature and density in Fig. 9). Other sections seem to have cut between boluses so that no very cold water has been observed. By the time the boluses have reached Lat. 60° N and have fallen to 3000 m or more, they have coalesced and become the North Atlantic Deep Water. This water lies below and to the west of the large mass of Faeroe-Iceland Ridge water which has turned the snout of the Reykjanes Ridge in Lat. 52° N at about 2700–3000 m sill depth and then spread out.

It is suggested that (1) variations from year to year in the number and size of boluses calved from the Denmark Strait, (2) consequent variations in the extent of upward displacement of water elsewhere, and (3) consequent variations in other processes not yet described, should be followed by variable enrichment of all the fishing grounds

of the northwestern North Atlantic. It is not suggested that the effect of one year's exceptional calving need be very great. If, however, a sequence of cold Arctic winters occurs, the accumulated effect may become considerable. This should be true not only for inorganic nutrients but also for organic regeneration products needed as accessory growth factors by certain plants and animals.

In areas of calving, a balance has to be struck between loss of nutrients by calving and cascading and the gain in compensation water. This is evidently a complex matter, so that no prediction is feasible for these areas.

From the Coriolis theorem I have always found it difficult to assess what the distribution of mass in a deep water or midwater current should be. Here we have two cases in the northern hemisphere where the heavier water is thrown to the right (where movement is strongest) and the lighter water to the left. This empirical rule applies throughout the course of the Iceland-Faeroe water and of the deep water around East Greenland. The slope of the 27.85 and 27.9 σ_t surfaces east of the Reykjanes Ridge was about 1 in 200. It is likely, however, that these west-seeking deep-water movements may be better studied in terms of the integrated vorticity equations of Stommel and Munk. These were developed for a wind-driven system of upper water currents, but perhaps they apply to deep thermohaline circulations also. Though ocean surface currents seem not to be frictionally bound to the bed of the ocean, may they not be so bound to these deep thermohaline currents?

If these views are true, even in part, answers to the problems posed by the fisheries biologists cannot be found only by study of shallow waters in which the fish live; knowledge of the deep circulation of the ocean is also needed.

To sum up, I have in part sketched a possible way in which climatic changes around Iceland may affect the landings of herring on the Plymouth fish market. The story is a consistent one but we still have to show that nature really works that way. We can never attain more data about the earlier years than we have now, so, for full confirmation we must wait until the climatic cycle comes full circle.

Summary. A cold heavy water appears to be created over the Faeroe-Iceland Ridge by mixing of Norwegian Sea water, East Icelandic water and North Atlantic Central water. A descending course for this new water mass is mapped along the southern flank of the Faeroe-Iceland Ridge and the eastern flank of the Reykjanes Ridge. In Lat 52° N it appears to turn into the Western Basin of the Atlantic. It is likely that the formation of this water mass is intermittent.

Available observations are not sufficient to establish this thesis with certainty. These concepts are developed further in a more recent paper.[3] The two papers together provide the basis for an explanation of the large fluctuations in biological productivity of the English Channel and adjacent waters.

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PHOTOSYNTHESIS AND PRODUCTIVITY OF LITTORAL MARINE ALGAE

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With the exception of *Sargassum*, the high seas traveller, and floating *Enteromorpha*, which occurs on quieter waters, practically all larger algae are anchored to the shore or shallow bottoms. The oceanographer would probably call them "land weeds" instead of "sea weeds." Nevertheless, they are bathed with sea water most or all of the time and so they partake of the ocean, even though the zone may be only a narrow one. While their total productivity is small compared to that of the entire ocean, yet, area for area, it can be very high. Indeed, perhaps the greatest apparent difference between littoral and planktonic algae is the immensely greater standing crop of the former, which often drapes itself thickly over rocks or forms heavy beds just offshore.

The other striking aspect of shore algae is their range of color, probably the greatest displayed by any group of plants (excepting autumn foliage of deciduous plants and trees). Not only do we find the familiar greens of *Ulva* and other Chlorophyceae, but usually the browns of still greater masses of algae, the kelps and rock weeds; and finally, there are those almost strictly marine plants, the littoral red algae, which range from bright pink to dark purple. Blue-green algae constitute so small a part of the total bulk that they are usually overlooked, though in the tropics they may contribute a conspicuous "black" zone to the beach rocks.

Partly due to the sheer mass of littoral algae, light absorption is extremely efficient along the shore. Since even a single sheet of algal cells, as in *Porphyra* or *Monostroma*, can absorb up to 60 or 70% of the incident light at certain wave lengths, it is not surprising that thicker algae do still better. When the green algae become massive, as they often do in the tropics (or as *Codium* does in the temperate zone), they appear almost black. The kelps so well produce this effect that their old name was Melanophyceae, meaning black algae. The red algae are often a deep purple. By repeated absorption, or by means of supplementary pigments which absorb regions of the

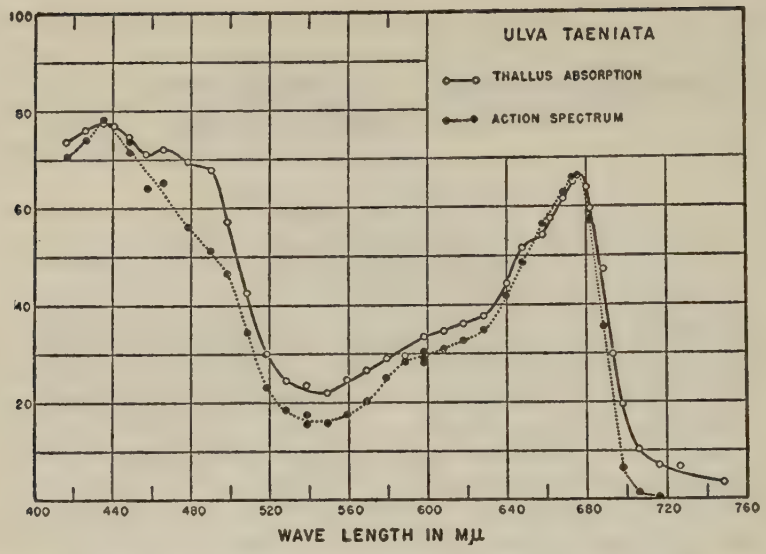

Figure 1. Light absorption and photosynthesis of a green alga (*Ulva*). Solid lines represent percent absorption by the thallus; dotted lines show relative photosynthesis.

spectrum not normally absorbed by land plants, the algae have become very efficient traps for light.

Green Algae. What are the absorbers? Primarily chlorophyll, of course—or rather the chlorophylls, since algae have four kinds. *Ulva*, with chlorophyll *a* and *b*, absorbs primarily at 435 and 675 mμ, which is where it also displays the highest photosynthesis. This seems so self-evident that it might not need emphasis were it not for less orthodox behavior now known among algae. And what of the other pigments, for even green algae have such? The ubiquitous β-carotene, as widespread as chlorophyll itself, and several carotenoids absorb broadly in the violet and blue end of the spectrum. In a chromatographed extract, about half of the light absorption between 400 and 500 mμ is due to these yellow pigments. This gives increasing interest to the photosynthetic response. Is it as good in the region of carotenoid absorption as it is in the red end of the spectrum, where chlorophyll alone absorbs? If so, must it not imply the participation of pigments other than chlorophyll?

Fig. 1, prepared in collaboration with Dr. F. T. Haxo (Haxo and Blinks, 1950), tells something about this, but it also poses a question. If photosynthesis is normalized at the chlorophyll red maximum (675 mμ in the alga), it then corresponds well with absorption through-

out the spectrum, even including the blue end, where (at least in extracts) carotenoids absorb about half of the light. Indeed, at the chlorophyll blue maximum (435 mμ), there is again coincidence. One can only conclude that the carotenoids are as effective light absorbers as chlorophyll and that it makes little difference, from the point of view of photosynthesis, whether green pigment or yellow absorbs the light. But there is a region at 480–490 mμ where photosynthesis falls appreciably below absorption. This might be regarded as accidental were it not found so consistently in photosynthetic studies. Emerson and Lewis (1942, 1943) found that the quantum efficiency of *Chlorella* and *Chroococcus* is noticeably down at this region and Tanada (1951) found that it is lower in diatoms likewise. We shall note shortly a similar depression in thalloid brown algae; perhaps it would appear in red algae except for interfering circumstances which will be discussed. It is not clear whether this depressed zone is due to ineffective absorption by some carotenoid or to an active "photo-oxidative" effect, due perhaps to riboflavin or cytochrome, which partially compensates photosynthesis. We have looked for anomalous "transition" effects in the time course of photosynthesis, in going from this region to red light, but we cannot find such in green algae. ("Transitions" do occur in red algae.)

Not all green algae utilize their carotenoid complement as well as does *Ulva;* many show lower activity in blue light. At times *Ulva* itself shows less, especially when the cells are preparing to form swarmers or gametes, which are rich in carotenoids (Haxo and Clendenning, 1953). Indeed, there are some green algae so burdened with carotenoids that they look orange or brick-red. Such is the brine flagellate *Dunaliella;* under very saline conditions it is loaded with β-carotene, but this seems to occur largely in oil globules, not in plastids; therefore, by all we know of photosynthesis, it is not topographically located so that it participates in effective energy transfer. Evidently it acts simply as a filter, cutting off blue light before it can reach the chlorophyll. It is not surprising that such algae cannot use blue light for photosynthesis (though they do for phototaxis).

This situation must be considered in many other cases, as in those where there are eye spots or even where the carotenoids might coat the outer parts of plastids, thereby absorbing much of the blue light before it can reach chlorophyll. It would be desirable to know more about the photosynthetic action spectrum of other yellow or orange algae; of special interest to oceanographers would be the dinoflagellates. Apparently they have not yet been studied in this regard.

Brown Algae. We turn now to the most successful employers of accessory pigments. These are the algae with brown pigments,

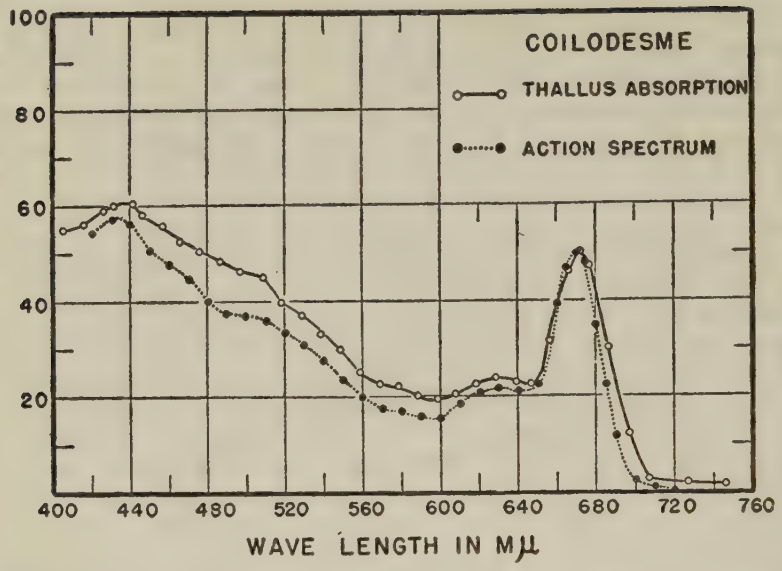

Figure 2. Absorption and photosynthesis in a brown alga (*Coilodesme*). Solid line; absorption; dotted line: photosynthesis.

the ubiquitous and productive diatoms as well as the rockweeds and giant kelps, which are yellow or brown when not indeed almost black. The brown color results from an increased absorption near the middle of the spectrum which takes out more of the green to which the eye is especially sensitive. This can be seen by comparing Figs. 1 and 2. It is due to a special carotenoid, fucoxanthin, which, while not greatly different from other carotenoids in extracts, extends absorption in the living plant up toward 560 mμ

Fig. 2 shows that in *Coilodesme*, as well as in *Ulva*, photosynthesis occurs at the chlorophyll maxima; it also occurs in the blue where carotenoids other than fucoxanthin absorb and to a considerable extent in the green wave lengths. Again there is some depression around 480 mμ, but, on the whole, absorption and activity are parallel, and we may conclude that the kelps have successfully extended the range of effective absorption. The same is true in the diatoms, as shown by Tanada's (1951) quantum efficiency determinations. Perhaps it is not surprising therefore that the kelps are the largest, and the diatoms the most abundant, of all algae; fucoxanthin was a useful invention. Yet this effective pigment is entirely restricted to aquatic and especially to marine algae. The higher plants have not used this

way of increasing light absorption but have employed thicker thalli or leaves.

Red Algae. Here we meet the most remarkable pigment situation in algae. Most collectors have seen red algae cast up on the beach. They are dark wine red when still alive but turn orange and display a bright fluorescence when injured and dying. Both the red and orange colors are due to the unusual pigment, phycoerythrin, which is restricted to this group and to the blue-green algae. It is a water-

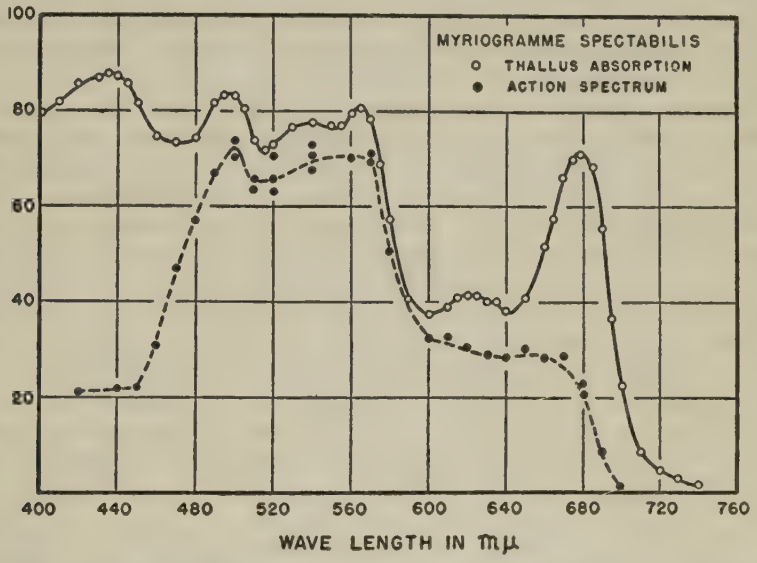

Figure 3. Absorption and photosynthesis in a red alga (*Myriogramme*). Solid line: absorption; dotted line: photosynthesis.

soluble chromoprotein of over 200,000 molecular weight, with a chromophore resembling the bile pigments. It is strikingly fluorescent in solution but much less so in the living alga. It and its close relative phycocyanin close the gap in chlorophyll absorption almost perfectly. With maxima at 495, 540 and 565 mμ (615 mμ for phycocyanin), red algae have much more complete absorption throughout the visible spectrum than either green or brown algae (Fig. 3).

Is this absorption effective? Many experimenters have shown that it is, since green light is far more efficiently used here than it is in the case of green or brown algae. Indeed photosynthesis is higher in the middle of the spectrum than that in the blue or the red regions! The action spectrum (Fig. 3) shows this in striking fashion. For

nearly equal absorption, phycoerythrin appears to be two or three times as effective as chlorophyll and four to five times as effective as the carotenoids of these algae.

Thus emerges the remarkable situation of a uniquely developed pigment which greatly increases the range of effective absorption but which, in doing so, inactivates some half to two-thirds of the accompanying and abundant chlorophyll. But chlorophyll is apparently still necessary; no photosynthetic plant occurs without it, and there is reason to believe that, even in the red algae, energy absorbed by phycoerythrin must be transferred to such of the chlorophyll as remains active and fluorescent. In view of this, the real question is, why is so much of the chlorophyll inactive?

A partial answer has been supplied by Yocum (1951), who grew red algae in red or blue light which was very free of the medium (green) wave lengths. After a week or so the algae increased their growth and it could be shown that photosynthesis was considerably enhanced in the red light; indeed, the action spectrum now displayed a good hump, if not a peak, at the region of chlorophyll absorption. Yocum also showed that even fairly brief exposures to green or white light again deactivated the chlorophyll. It is apparently the absorption of light by phycoerythrin itself which deactivates chlorophyll, but it is not known whether this results from some photodynamic effect or from pigment competition for enzymes. So far we have succeeded in reactivating chlorophyll by red light only in the more primitive *Porphyra* and *Porphyridium.* In the higher red algae the inactivation is apparently permanent. It is indeed remarkable that plants should have developed a useful new pigment only at such a price. It would seem that any advantage derived from phycoerythrin's midspectral absorption could be obtained if chlorophyll were still effective. The incompatibility seems very wasteful, but nevertheless it is a fact and deserves further study.

Relation to Depth. We may now ask: Is there a relation between pigmentation and habitat? Algologists have speculated for years about zonation, with emphasis on the ability of red algae to live at greater depths than green or brown ones. There is no doubt that they do so in some regions. And good reason can now be assigned to this ecological fact, for, with the absorption of blue and red light in coastal waters and consequent deeper penetration of green light, a fact well established by Clarke and others, there is at moderate depth a habitat favoring red algae.

Levring (1947) has calculated the ability of algae to photosynthesize at different depths. In green algae, at 10 m depth, photosynthesis is low indeed, and at 20 m it is practically nonexistent because the

highest rates of green algal photosynthesis lie in regions of the spectrum most absorbed by coastal waters. On the other hand, red algae showed good photosynthesis at 10 m and quite appreciable amounts at 20 m. This naturally follows from their higher photosynthetic rate in green light (Fig. 3); the absorption of red and blue light by coastal water is no disadvantage, since this is used poorly by red algae. One might also predict a slight depth advantage for brown algae due to their enhanced photosynthesis in green light (absorbed by fucoxanthin). But most of the kelps (and certainly the diatoms) are littoral and floating.

One should, in fact, not be too impressed by these relations. Many things may influence zonation. For example, if respiratory rates were very low, algae could probably survive in the dim light of 20 to 40 m; and it is true that red algae have a rather low respiration. On the other hand, many kelps have a high respiratory rate and tend to be found in bright light. Nor does actual zonation correlate well with the pigment complement; on the Pacific Coast, three of the highest growing algae which are found almost in the spray zone are red: *Porphyra perforata*, *Gigartina papillata*, and *Endocladia muricata*. Two common brown algae, *Fucus* and *Pelvetia*, occur a little lower down; only below these do we find common green algae like *Ulva*, *Cladophora*, and *Spongomorpha*. This is exactly the inverse of the classical green, brown, red sequence. Other factors, such as massiveness and dessication resistance, and perhaps infra-red and ultra-violet tolerance as well, may govern zonation more than pigmentation—at least in the upper levels.

Quantum Efficiencies. All of the action spectra shown above are relative rather than absolute. It might be argued that chlorophyll activity in red algae is not *low* but rather that phycoerythrin activity in them is extremely *high*. We have, therefore, investigated the quantum efficiencies of representative green, brown and red algae freshly gathered from their positions of growth (Yocum and Blinks, 1954). The values have considerable ecological interest, since they supply information on this important point for "wild" or natural plants, thereby supplementing the almost universal study of the cultivated, highly selected, and practically "domesticated" *Chlorella*. The method, completely different from the one used for action spectra, lends independent confirmation to the latter. Table I shows the results. No records are broken as to efficiencies. There are no 4-quantum values (not even 8) whereas 10 or 11 up to 12 or 14 absorbed quanta are required to release a molecule of oxygen. The efficiencies are essentially the same throughout the spectrum for both green and brown algae. While not high, they are at least

rather uniform. This is consistent with the equal participation of all pigments as seen in the action spectra. In the red algae, the highest values are found in the middle of the spectrum, just where action is greatest. This would be expected, but note that the value, 14 to 15 quanta per molecule of O_2, is no higher than that found throughout the spectrum with *Ulva*. *Consequently, phycoerythrin is as efficient in the red algae as is chlorophyll in the greens.* As a corollary, we would expect and did indeed find that the efficiency of red algal chlorophyll is low, being only half as high as that in *Ulva*. The carotenoid absorption is extremely ineffective (35 to 50 quanta per molecule) but quite in line with the action spectra.

It would be interesting to know how the efficiency of these littoral algae compares with that of phytoplankton from the open ocean.

TABLE I. Number of Absorbed Quanta Required to Release One Molecule of Oxygen (After Yocum and Blinks, 1954)

	Wave length ($m\mu$)				
Algae:	436	500	560	620	675
Green					
Ulva lobata	14	18	23	16	12
U. lactuca	16	14	13	15	15
Brown					
Ilea fascia	10	11	11	11	10
Red					
Porphyra Nereocystis	37	16	14	15	25
P. naiadum	35	24	13–17	18	25
Delesseria decipiens	26	16	16	20	32

In view of the somewhat better behavior of the unicellular *Chlorella* and considering the fairly high efficiency found by Tanada (1951) in diatoms, which was not too different from that of the brown alga shown in Table I, it may be predicted that planktonic algae will equal littoral ones in absolute efficiency.

Littoral Productivity. Long term (seasonal or yearly) production of organic matter has not been studied as thoroughly in littoral algae as in algae of the open ocean, though Printz (1950), Sargent and Lantrip (1952) and a few others have given interesting figures for large algae. The standing crop of attached algae, as mentioned earlier, is often immense; the great masses of coastal algae are several orders of magnitude denser than phytoplankton. Compared to the thick tangled beds of kelp in Scotland, in California, and in similar spots, the open ocean just beyond looks like a transparent desert. We know part of the reason, of course—there is far more efficient

grazing of phytoplankton by animals. But may this difference be due also to greater inherent production? Does anchorage give some advantage in productivity?

Some idea of this can be obtained from the standing crop, that is, from harvests of commercial kelps, especially of those which are annuals. Estimates of 13 to 14 tons of *Laminaria* per acre per year have been given in Scotland (Inst. of Sea Weed Research, 1953). This calculates to some 10 kg/m², which is roughly the same as that which we have found in California for the annually renewable portion (blades) of *Laminaria* (4.4 kg/m²). This is the greatest standing crop we have found. Other values, obtained during the past year by Dr. Lars Carpelan, are shown in Table II. The first column shows

TABLE II. Productivities of California Algae (Dry Weight) as Determined by Carpelan

	Standing crop g/m^2	*Production* $g/m^2/day$	*Time for crop* *days*
Ulva	70	3–7.2	10–23
Porphyra	300	11–21	15–26
Gigartina	750	54	15
Iridophycus	760	19	40
Egregia	800	25	32
Alaria	1800	14	130
Fucus	2130	19–42	50–112
Pelvetia	4240	35	120
Laminaria (*blades*)	4400	66	66

the standing crop in autumn and spring for some abundant green, brown and red algae. These range from 70 g/m² (*Ulva*) to the 4.4 kg/m² (blades only) just mentioned for *Laminaria*. Two red algae approach 1 kg; several browns exceed 2 kg. Compare these figures with phytoplankton—only ½ g/m^3! Even when correction is made for euphotic depth, the amount under a square meter of sea surface seldom exceeds 10 or 20 g. Why this factor of 100? Are the coastal animals such poor grazers? Or is production really higher on shore?

Most of the algae listed here tend to be annual. *Laminaria* drops its blades around January and grows new ones of good size between March and June. *Alaria* disappears entirely in winter but becomes six feet long by June. The giant kelp, *Nereocystis*, is an annual, becoming 10 m in length during the growing season. This means that many of the listed algae produce in a few months at least the standing crops shown in Table II, and they probably produce a great deal more which is broken loose, grazed or possibly leached away.

These figures are necessarily minima. Therefore, production during the growing season is at the rate of ½ to 1 kg organic matter/m²/*month*. Compare this estimate with the figures tabulated in Sverdrup, *et al.* (1942), which show values of fixed carbon in the ocean to be this high *per year*, at most, and running down to 5 or 10 g in less productive waters.

Such determinations, of course, were done mostly by oxygen evolution methods. How does the latter compare in the littoral algae? Column 2 of Table II indicates organic production in the standing crop, as estimated by oxygen evolution of samples in closed bottles. The spread observed in standing crop is not found here. *Ulva* produces almost half as well as *Alaria*, though its crop is only 4% as great; *Gigartina* produces as actively per square meter as does *Laminaria*, though with only 1/6 the crop. Both *Laminaria* and *Alaria*, remarkably resistant to both surf damage and grazing, can go ahead accumulating substance for many months. *Ulva* and *Gigartina* are much more delicate, yet they apparently duplicate their standing crop, at the indicated photosynthetic rate, in 10 to 20 days (depending on cloudiness). *Laminaria* would take two months, *Alaria* four. These production rates (g/m²/day) are the average of determinations obtained in autumn and early spring. Figures now at hand for summer growth indicate even greater production.

The eye, therefore, does not deceive. These tangled masses of kelps, often a foot deep on rocks or covering acres offshore are indeed productive. The littoral zone, narrow though it be, is a region of great carbon fixation. (Perhaps, like all specialists, its productivity is greater the narrower it becomes.) There does seem to be real advantage in trapping the sunlight in a few layers of densely packed cells rather than in competing with the absorption of water itself, as phytoplankton must. Doubtless there is also a nutritional advantage in being washed by the waves instead of floating with them.

It is a paradox that one of the largest kelps, *Nereocystis*, despite its great individual length and bulk, does not produce the greatest amount of organic matter per square meter. In October 1953, Carpelan estimated 292 *Nereocystis* heads in a small bay which had an area of about ½ acre, or some 2000 m². Analysis gave only about one plant per 10 m² and by dry weight of representative samples, only about 37 g organic matter/m²—not as much as *Ulva*! There may be disadvantage in too great size, thickness of tissue, and depth of growth, with the necessity of translocation.

Nutrient Cycle. Granted high productivity and low grazing on littoral algae, why then is the ocean not thick with kelp? How does this wealth of organic matter return to circulation after the active

growing season? Largely by decay. In the winter, and even through much of the year, littoral algae are cast ashore, sometimes in immense masses. Here they die in the sun, are buried in the sand, and within a few days or weeks they are completely decayed. Bacteria, protozoa, small crustacea, are soon at work in the mass, and it is shortly back in animals. Although carbon is hoarded for a season, in a year it is back in circulation and the whole cycle can repeat. The grazing was simply delayed. Possibly many of the nutrients remain in the littoral zone and account for its high productivity. This would appear to be a subject worth further investigation.

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PRODUCTION OF ORGANIC MATTER IN THE OCEANS

By

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As on land, so in the sea, autotrophic plants are the basis of all life. Sessile plants live on a narrow fringe along coasts only, so, if we wish to consider the amount of matter produced by plants of the sea, we must confine our attention to planktonic algae, which are found everywhere in the upper oceanic water masses. It is the organic matter synthesized by phytoplankton out of purely inorganic substances by means of light which directly and indirectly serves as food for all organisms in the sea, from the smallest bacteria to the largest whale.

The first investigations of production of matter by phytoplankton in a coastal area were made 30 years ago by Dr. Atkins of the Plymouth Laboratory, England. He proceeded partly by determining the loss of carbon dioxide and partly by observing the loss of phosphate under one square meter of surface from the end of winter to the height of summer. Since the amounts of these substances which disappeared must have been used to build up vegetable matter, a minimum value for the production of matter in six months could be calculated from the values found. Atkins worked in the English Channel, and his method has been used in some other coastal waters too.

About 1930 the Norwegian planktonologist Gran introduced a direct method for measuring the production of matter by phytoplankton. In Gran's method, water samples from various depths are put into bottles and then placed at the different depths from which they were taken, normally for 24 hours. Oxygen production, which is an expression of photosynthesis, is determined by measuring the amount of oxygen present at the start and end of an experiment. The method has been used relatively little, since it is suitable only in areas with a large production of organic matter.

When the Danish Galathea Expedition put to sea in October 1950, one of the main items on its program was the investigation of organic productivity in all oceans. Since none of the methods mentioned seemed to be suitable for normal oceanic waters, it was important that the expedition be provided with equipment for determin-

ations by a new method. This method, which has taken the radioactive carbon isotope C^{14} into its service, proved applicable with equal certainty whether the productivity was high or low. A basis has now been established for an estimate of production of matter in the oceans, at least on broad lines.

Two slightly different modifications of the Carbon^{14} method were used. In the first modification, samples of water taken from various depths in the photic zone were transferred to bottles with glass stoppers. Following addition of C^{14} in the form of sodium bicarbonate, the bottles were suspended at the depth from which the samples were taken and left there for a definite time, for instance, from noon until sunset. At the end of this exposure period, the plankton was filtered off and the amount of C^{14} assimilated was determined by measuring the β-radiation. To calculate the total carbon assimilated, it was necessary to multiply the amount of C^{14} found by a factor corresponding to the ratio between $CO_{2(total)}$ and $C^{14}O_2$ in the water. In view of the fact that C^{14} is not assimilated at quite the same rate as C^{12}, a correction for the isotope effect had to be made.

In the second modification, the bottles containing water samples from different depths were placed in a water bath which was illuminated by a definite light intensity. The penetration of light in the sea was determined at the time of collection. The production of matter beneath a unit surface was calculated by means of a formula derived from comparison of results from stations at which observations were made simultaneously by the two modifications. The first is the most accurate of the two modifications, but it takes about 20 times longer than the second.

On this occasion I will not discuss details of the method. Instead, I will give some of the main results obtained on the GALATHEA.

The most remarkable feature is a marked correlation between rate of production and hydrographic conditions. In the open sea, high productivity is found only in regions with a pronounced movement of water from deeper layers to the surface.

Fig. 1 gives the rates of organic production in a section from New Zealand to California. The direction of the route was northwestward to Honolulu, thence straight to San Francisco. The neritic station at the mouth of Hauraki Gulf, New Zealand and the last station in the coastal water outside San Francisco are included to give suitable contrast to the other stations, all of which are truly oceanic. These two neritic stations represent coastal areas with high productivity—about 0.6 g C/m^2/day.

The CARNEGIE's last cruise in 1929, together with the DANA's cruise in 1928–1930, gave valuable information on the hydrography

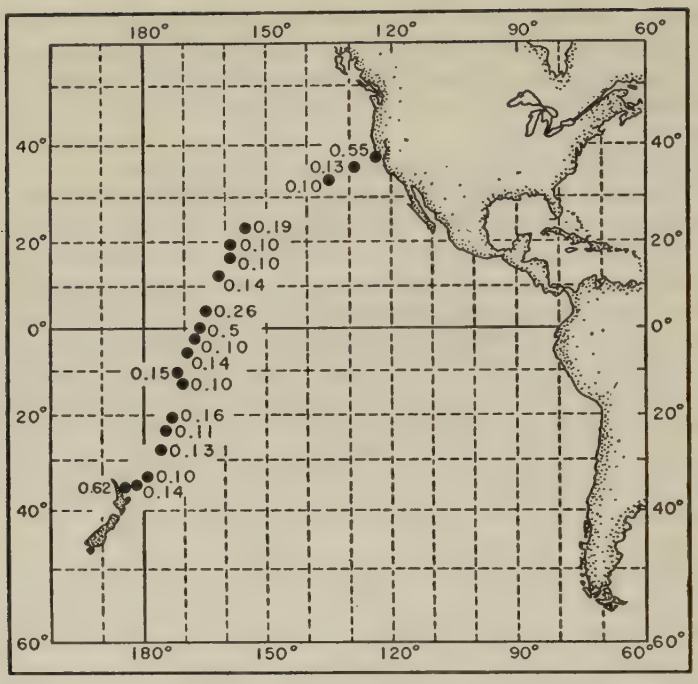

Figure 1

of this area. In the east, between the North and South Equatorial Currents, there lies the remarkably well developed Equatorial Counter-Current. The CARNEGIE found two regions of divergence in which the ascent of water from medium depths to the surface could be clearly recognized; one is situated in the South Equatorial Current near the Equator, the other on the northern boundary of the Counter-Current. In these regions concentrations of slightly more than 0.4 μg-at P/l were found at the surface, and net catches of plankton were also rich here. Hydrographic and chemical conditions as found by the GALATHEA were rather similar to those found by the CARNEGIE.

The rate of organic production was fairly uniform at the GALATHEA'S oceanic stations on this section. With the exception of stations near the divergences created by the Equatorial Counter-Current, the rate varied only between 0.10 and 0.19 g C/m^2/day. Only two stations were made in the region influenced by the divergences; the rates of production there were 0.26 and 0.5 g C respectively.

The thickness of the photosynthetic layer at the oceanic stations varied mostly between 95 and 120 m except for stations in or near the Counter-Current, where it was 78 and 85 m.

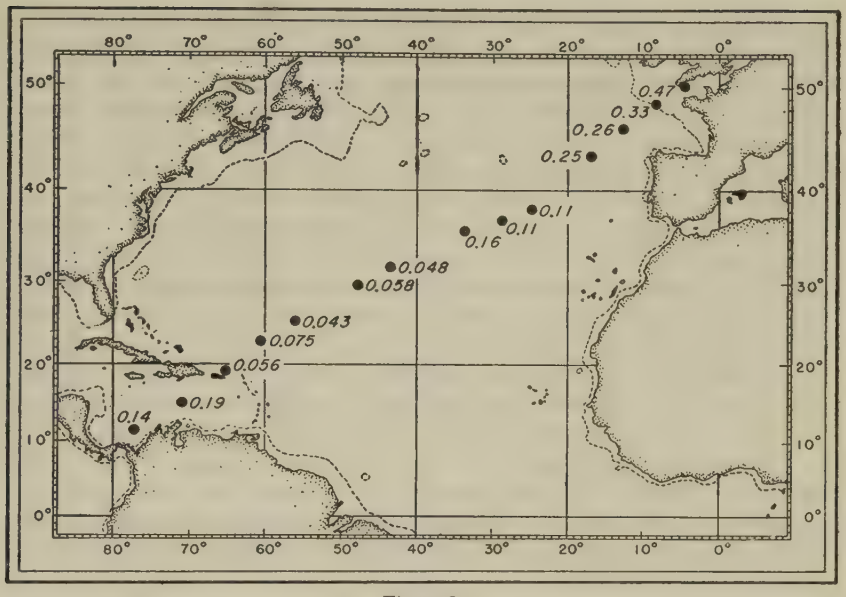

Figure 2

The oceanic surface water encountered on the GALATHEA section must be characterized as of medium age whereas that in the region near the mentioned divergences must be regarded as fairly new. An organic productivity of 0.10–0.19 g C/m²/day is thus characteristic of tropical and subtropical regions having surface water of medium age. I am here using the term "age" of water in the manner originally introduced by Sverdrup and Allen.

Fig. 2 shows the rates of organic production on a section from Panama to the English Channel, which was run during May and June 1952. The first two stations are situated in the southern part of the Caribbean Sea, the next two in the Antilles Current (a westward continuation of the North Equatorial Current), three in the Sargasso Sea, six in the North Atlantic Current (the last one on the continental slope), and finally, one at the mouth of the English Channel.

The hydrographic conditions of the North Atlantic are well known. The surface water in the Caribbean Sea has its origin in the North Equatorial Current, which, at its western end, carries fairly old surface water. Organic productivity is 0.14–0.19 g C. Production in the Antilles Current is poor, being only 0.056–0.075 g C, which accords with the rather old age of the surface water.

About the lowest oceanic productivity found anywhere by the GALATHEA—0.043–0.058 g C—occurred in the Sargasso Sea. In this

big anticyclonic eddy in mid-Atlantic the surface water is old, and here the same water circulates for a fairly long time. The surface water receives contributions from the sides, partly from the western end of the North Equatorial Current (carrying old surface water) and partly from the uppermost layers of the Gulf Stream. Owing to the anticyclonic form of the eddy, situated as it is in the northern hemisphere, there is a tendency for surface water to descend near the centre. This effect is seen in the distribution of the nutrient salts below the photosynthetic zone. Concentrations above 0.4 μg-at P/l are first found several hundred metres below this zone, which, in the Sargasso Sea, reaches down to about 120 m. Thus only quite minimal amounts of nutrient salts are conveyed to this zone by mixing with water from below the photosynthetic zone. It is readily understandable, therefore, that the rate of production in the Sargasso Sea is low. This sea has long been regarded as a poor region, and the blue water there has been described as an oceanic desert. The paucity of plant production is reflected also in the rate of animal production. The smallest volumes in zooplankton reported by the Danish DANA Expedition round the world were found in the Sargasso Sea.

The stations in the North Atlantic Current represent a region of typically subtropical oceanic surface water of medium age. The rate of organic production varied between 0.11 and 0.16 gC. As one approaches the European continent organic productivity increases, probably due to the activity of eddies found there.

Finally, a productivity of 0.47 g C was found in the mouth of the English Channel. If production there occurs at that rate from March to June, then the result will be a productivity of the same order of magnitude as that estimated by the Plymouth Laboratory from the decrease of phosphate in spring until summer.

Fig. 3 shows the GALATHEA's results for the most productive area visited—the southern part of the Benguela Current, which is known to be one of the most productive areas in the oceans. This Current flows along the west coast of South Africa and is particularly conspicuous between the southern extremity of Africa and Lat. 17°–18° S. Under the influence of prevailing southerly and southeasterly winds, the surface layers are carried away from the coast and are replaced by upwelled water from moderate depths during most seasons of the year. These hydrographic conditions give rise to an enormously rich phytoplankton, since the upwelling water is rich in nutrient salts and establishes excellent conditions for plankton production. In the Benguela Current we find typically new surface water. According to the GALATHEA's measurements in the Current, the rate

of organic production varied between 0.46 and 2.5 g C/m²/day (in Walvis Bay, 3.8). However, we must expect considerable variation in productivity in a locality such as the southern part of the Benguela Current because, for example, of the shape of the coast and the weather conditions peculiar to it.

It is now well established that the factors governing production of phytoplankton in the sea are, in the main, as follows: a) supply of nutrient salts, b) light, c) temperature, and d) grazing. The relative importance of these factors, however, is not so well known.

The investigations made on the GALATHEA Expedition have furnished weighty evidence to support the fact that the supply of nutrients—manifested by the age of the surface water—is by far the most important factor governing the production of phytoplankton in the tropical and subtropical parts of oceans.

Nutrient salts are supplied not only by deeper waters but by horizontal currents as well, and nutrients carried up to the surface in one particular place may therefore influence other regions too if such regions are connected by horizontal currents. At the moment it is impossible to say how long a phosphorus atom carried up into the photosynthetic layer is likely to stay there and in how many phytoplankton cycles it may participate before being lost by sinking below this layer. Local variations will certainly occur. It can be demonstrated, however, that a particular surface water, rich in nutrients and therefore capable of high productivity, becomes poorer and poorer as it is transported by a horizontal current to regions where no other enrichment of the surface water takes place. The North Equatorial Current in the Atlantic offers a typical example. At the start of this Current, near the African coast, upwelling of water rich in nutrients takes place and organic production is rich. The western part of this Current (the Antilles) is characterized by low productivity, and the mid-Atlantic eddy, which is a further continuation of this Current, exhibits still lower productivity.

In planktonic algae as in all other autotrophic plants, variations in temperature strongly influence the rate of respiration and the rate of photosynthesis at a high light intensity. This does not necessarily mean, however, that the rate, measured per dry weight, is much lower in the arctic than in the tropics. In the arctic, a higher concentration of the enzymes that participate in respiration and photosynthesis may counteract the depressing effect of low temperature. In this respect marine phytoplankton has not been studied. Several authors have given examples of cases in which the rate of respiration of a given species, when subjected to the same temperature, is less for individuals collected from a warmer area than for those col-

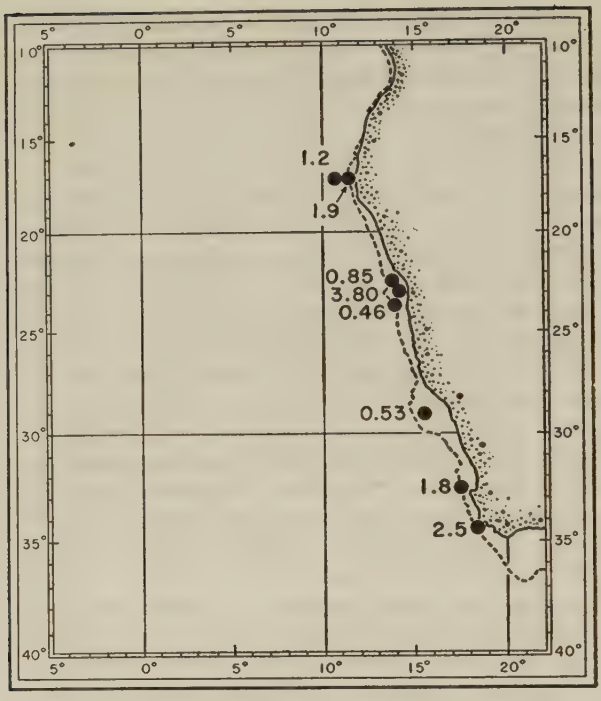

Figure 3

lected from colder waters. If this occurs with individuals of the same species it is probably still more pronounced when cold and warm water species are compared.

The extent to which productivity is directly influenced by temperature is not known, but indirect influence of this factor is perhaps the more important. Low temperature often seems to have a pronounced effect by inhibiting the activity of bacteria. Possibly an increase in temperature may accelerate regeneration of the nutrient salts with the result that organic production is also accelerated.

In highly productive regions having a rich supply of nutrient salts, light intensity is the most important limiting factor. It is also limiting in high latitudes during winter, in regions that lack stability of the water column, e.g., the Antarctic Ocean during spring. However, in tropical and subtropical areas, outside those localities with a pronounced upwelling, light intensity seems to be of little importance as a limiting factor.

Let us consider a tropical or subtropical station at which the depth

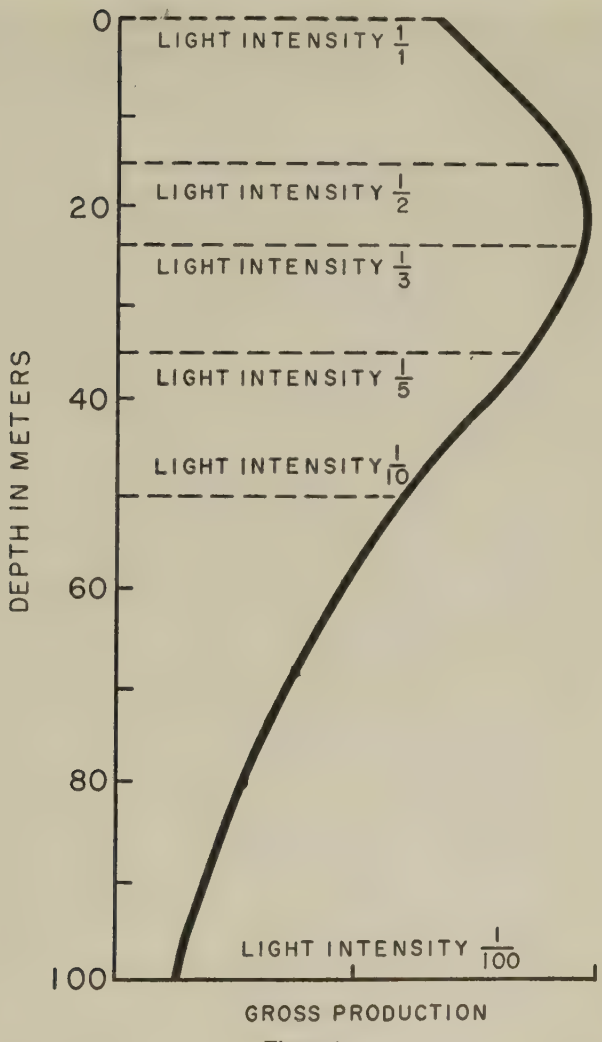

Figure 4

of the photosynthetic layer is 100 m. If the phytoplankton are evenly distributed in the layer and if the light absorption is the same at all depths, then we obtain the hypothetical curve shown in Fig. 4. This represents the daily relative rates of total photosynthesis at different depths on a clear day. The maximum rate is found at a depth of about 20 m, where the average of the blue and green

part of the light falling on the surface is reduced to nearly one-third. Since the rays of the red part of the spectrum are completely absorbed in the upper 5 to 10 m, we may disregard this part of the light. Light intensity, being too high near the surface, causes a reduction in the rate of photosynthesis.

If the light falling on the water surface is reduced throughout the day to one-third, then the rate of photosynthesis at the surface is the same as that which takes place at a depth of 24 m on a clear day. With a decrease in light intensity to one-third, the photosynthetic rate per unit volume of surface water (g C/m^3/day) thus increases by nearly 50%. However, the photosynthetic layer is now only 76 m deep. The curve for light intensity one-third is identical with the curve which would appear if the curve for light intensity 1/1 at the surface is shifted bodily 24 m upwards. The rate of total photosynthesis measured per square metre (g C/m^2/day) is thus about two-thirds of that measured at the light intensity 1/1 at the surface. By reducing the light intensity at the surface to one-tenth, the curve has to be moved bodily 50 m upwards, which reduces the depth of the photosynthetic layer to 50 m. The rate of total photosynthesis at the surface expressed as grams Carbon per cubic meter per day decreases about 15% and the rate per square metre decreases about 75%.

A day with light intensity of only one-tenth of that of a clear day is in fact very dark; for example, one on which rain falls throughout the day. Only in such dark weather does a slight reduction of the photosynthetic rate (per unit volume) occur at the surface. The rate of photosynthesis below a unit area is affected somewhat more by weather conditions, but the rate is seldom reduced by more than 50%. On only extremely dark days is the rate reduced by about 75%.

If other hydrographic conditions are the same throughout the year, presumably no significant variations in productivity are to be expected in tropical areas of the oceans. In the subtropics the variations between summer and winter are believed to be fairly small. In the Sargasso Sea, at 30° N, the average amount of radiation from sun and sky which reaches the sea surface in the darkest month of the year (December) is about 50% of the amount in the brightest month (August). According to the figure, assuming other factors equal, the rate of production per unit area in the darkest month is about 80% of that in the brightest month.

The effect of grazing will not be discussed here. However, various experiments were performed aboard the GALATHEA to obtain information regarding this factor. Grazing was found to be fairly important.

Investigations carried out during the GALATHEA Expedition have shown that the amount of organic production in tropical and sub-

tropical parts of the oceans is strictly dependent on hydrographic conditions in the locality. Although only one experiment was performed at each oceanic station, there is every indication that the results provide a reasonably correct picture of the productivity of the region. Of course it must be stressed that this generalization is by no means applicable to temperate and arctic waters, nor does it apply in every case to many tropical and subtropical coastal waters.

From the amount of organic production it is possible to distinguish four different classes of oceanic regions. These are applicable to tropical and subtropical areas of the oceans only, with graduations between them, of course.

(1) Regions with a considerable admixture of new water to the photosynthetic zone, new water being that which is rich in nutrient salts originating from medium depth. The daily organic production is from 0.5 to about 3 g C/m²/day. An example of this class is the southern part of the Benguela Current.

(2) Regions with a fairly steady admixture of new water to the photosynthetic zone. Daily organic production is about 0.2–0.5 g C. This may be exemplified by regions in the neighborhood of the divergences caused by the Equatorial Counter-Currents in the different oceans.

(3) Areas without a pronounced admixture of nutrient-rich water from below; however, some admixture, though of minor importance, may occur through turbulence. The daily production is 0.1–0.2 g C. By far the largest parts of the tropical and subtropical regions of the oceans belong in this category.

(4) Areas with typical old surface water in the photosynthetic zone. This water, originally rich in nutrients, has been transported from a distant region, and in the course of time since it entered the photosynthetic zone there has been a decrease in nutrient content. Turbulence is of slight importance only, since nutrient-rich water is present only far below the photosynthetic zone. The rate of production is about 0.05 g C. An example of this is the Sargasso Sea.

I am aware that much too little is known about the productivity of the arctic, antarctic, and temperate seas, where seasonal changes are pronounced, but nevertheless I have ventured to estimate the average daily gross production in all seas as about 0.15 g C assimilated per square mile. According to this, the net production for the whole hydrosphere is about 1.5×10^{10} tons of carbon per year, which is the same as the general estimate for production on land.

On the GALATHEA Expedition a high rate of organic production was always found in conjunction with a shallow photosynthetic layer. In Walvis Bay (South Africa), where the highest rate of

production was recorded (3.8 g C/m²/day), the photosynthetic layer was only 0.8 m deep.

The data for 184 GALATHEA stations are presented in Table I. The numbers are those from stations which fell within each category of production rate and depth. They reveal a marked negative correlation between the vertical extent of the photosynthetic layer and the rate of organic production. For a depth of 100–120 m, none of the stations had a rate of production above 0.24. For a depth of 75–99 m, none had a rate above 0.49, the main part showing a rate between 0.11 and 0.24. For a depth of 50–74 m, none had a rate above 0.99, but about a quarter of the stations in this category showed rates between 0.05 and 0.99. For a depth of 25–49 m, a few stations had a rate of production above 1.0, but for the most part they were characterized by rates between 0.25 and 0.99. The stations with

TABLE I. CORRELATION BETWEEN THE VERTICAL EXTENT OF THE PHOTOSYNTHETIC LAYER AND THE RATE OF GROSS PRODUCTION

Rate of gross production	*Vertical extent of photosynthetic layer (m)*				
(g C/m²/day)	*0–24*	*25–29*	*50–74*	*75–99*	*100–120*
>1.00	7	3	—	—	—
0.50–0.99	6	15	12	—	—
0.25–0.49	6	15	17	9	—
0.12–0.24	2	6	15	37	12
0.0 0.11	2	1	—	9	12

the shallowest photosynthetic layer (less than 24 m) belong essentially to the category with a rate of production above 1 g C/m²/day.

However, it must be borne in mind that some coastal stations may be characterized by a thin photosynthetic layer and an insignificant rate of organic production because of decreased light penetration due to suspended clay and other material. The stations off the mouth of the Congo River provide typical examples of this kind. But in general a marine area with a shallow photosynthetic layer is highly productive. On the other hand, if the photosynthetic layer is deep, the rate of production is always low. In the clearest ocean water, as in the Sargasso Sea where the depth of the photosynthetic layer is about 120 m, it is of considerable interest to estimate the rate of production which can be expected theoretically in such waters. This can be done by using the results from experimental plankton work.

In various countries during the last few years important work has been performed to determine the maximum possible rate of

organic production by planktonic algae grown in large basins in a pilot plant under natural light conditions. In these experiments light intensity is almost the only limiting factor. All other factors, e.g., the concentration of nutrient salts, are made as optimal as possible. The most successful experiments yield a net production rate of slightly more than 10 g dry matter per square meter per day. This means a gross production of about 5 g C/m²/day, which is about 30% more than that which occurs under natural conditions in Walvis Bay. The depth of the algal suspension in the basins is only a few centimeters, and the concentration of the algae (mostly *Chlorella*) is so high that nearly all the light that penetrates the surface is absorbed by the algae. The light absorption in the thin layer of water is without any importance.

In the ocean, however, when the depth of the photosynthetic layer is about 120 m, the algae absorb only a small part of the light and utilize it for the production of organic matter. Thus, most of the light is absorbed by the water itself. It would take too long to go through all the necessary computations, but briefly it can be stated that, theoretically, a maximum rate of production of only one-twelfth of the rate in the basin of the pilot plant is possible, that is 0.4 g C/m²/day.

However, this value is still too high. The phytoplankton in oceanic regions of very transparent water grow under conditions far below the optimum that occurs in pilot plants. In the first place, the concentrations of nutrient salts are not optimal. If growth conditions were better, the increasing population of algae would soon render the water less transparent, thus diminishing the depth of the photosynthetic layer. Therefore, in oceans with a photosynthetic layer more than 100 m thick, it is not astonishing to find that the maximum rate of gross production is only 5% of the rate possible in the pilot plant and that the average rate is only about 2% of this rate. The waters of the Sargasso Sea are transparent because of the insignificant rate of organic production that occurs there as a result of the poor supply of nutrient salts.

In the foregoing, the rate of production has been stated per square meter of surface, since only in that way can the different regions of the sea be compared *inter se*. The rate of production however, as measured by volume, is also of considerable interest. Undoubtedly it is of importance to grazing animals whether plankton are concentrated in a shallow layer or dispersed through a deep layer. Moreover, although the planktonic algae are for the most part fairly evenly distributed throughout the photosynthetic layer, this is by no means always so.

Calculated per unit volume, the most productive station on the GALATHEA Expedition (Walvis Bay) yielded a rate of production at the surface which was 14,000 times the rate at the surface of the Sargasso Sea. Calculated per unit area, however, the rate at the Walvis Bay section is only 80 times higher. The differences in the depths of the photosynthetic zones, 0.8 and 121 m respectively, account for the differences in the rates.

From the GALATHEA investigations, it is possible to state that the laws governing organic productivity in tropical and subtropical parts of the oceans are not very complex. This does not mean, of course, that there is no reason to make further investigations in this field. Quite the opposite; it is an argument for intensifying such studies. It is important to possess detailed knowledge of organic productivity in the sea for all parts of the earth in order to understand better the conditions affecting the production of fish and other marine animals. Regions of primary interest to fisheries are mostly near the coasts, and organic production in coastal areas is often of a somewhat more complicated nature than that occurring in tropical and subtropical parts of the open oceans.

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MODERN ASPECTS OF MARINE LEVEL-BOTTOM ANIMAL COMMUNITIES

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Modern biologists are entangled in such numerous problems that their total staff is far too small to tackle but a fraction of them. It seems reasonable, therefore, that we should concentrate on the solution of those problems which may help most to widen our general view. This is the reason why I propose to call your attention to the advantage of studying marine level-bottom communities in the sense described by C. G. Joh. Petersen (1911, 1913, 1918). It is a field which now seems to be ripe for world-wide teamwork and which at the same time has economic implications, since it concerns the feeding grounds of most of our bottom-dwelling food fish, especially of the cod- and and flounder-groups.

The level-bottom areas of the sea are among the most uniform flats on our globe, and from these areas extensive series of quantitative level-bottom samples, procured by aid of the Petersen grab and similar instruments, have been brought together during the last 35 years: from the high arctic areas to the tropics and from the shore to more than 10,000 m depth (Spärck, 1935, 1936a, 1937; Jones, 1950; Thorson, 1952). Roughly, this comprehensive material has demonstrated a most remarkable fact, that the same types of level-bottom substrata at similar depths are quantitatively dominated by invertebrates belonging to the same genera but to different species.

Different communities, always including species of the lamellibranch genera *Macoma*, *Cardium* and *Mya* as dominants and always associated with mixed bottoms just below the tidal zone, are known from high-arctic as well as boreal-atlantic seas, from the Pacific coast of the United States, from Japan, from East Africa, and so on, thus making a chain of "parallel" *Macoma* communities that replace each other in accordance with latitude and temperature. Chains of parallel *Venus* communities, always associated with sandy bottoms in shallow water and dominated by the lamellibranch genera *Venus* and *Spisula* or *Mactra*, are known from East Greenland, the North Sea, the Mediterranean, and the Persian Gulf. Chains of parallel communities of

brittle stars, associated with muddy bottoms in somewhat deeper water and dominated by replacing species of the genera *Amphiura*, *Amphiodia*, and *Amphioplus*, are known from the North Atlantic, the Black Sea, the Persian Gulf, Japan, New Zealand, the coast of Chile, and the Vancouver region.

Having here given examples from sandy, mixed, and muddy bottoms, we find that each series of parallel communities shows uniform features qualitatively as well as quantitatively everywhere.

Of approximately one million species of recent animals reported thus far (Pratt, 1935; Muller and Campbell, 1954; Hyman, 1955), only about 16% are marine. However, more than 80% of the animals inhabiting the sea bottom, which is only a fraction of the total marine fauna, are associated with rocks, stones, vegetation, coral reefs, etc. This means that level-bottom areas, though covering more than half of the surface of our globe, are inhabited by only 2–3% of all recent species of animals; this fact further demonstrates the simplicity of these areas as an ideal field for ecological work. Among these 2–3% only a small fraction, comprising astonishingly few genera and species, will prove to be quantitatively dominant on the level bottom, and these few genera one day will be the geological guide fossils for our period as were the ammonites, belemnites, and trilobites in prehistoric seas.

This uniformity and paucity in number among level-bottom species, as compared with the fauna of marine rocks, reefs, and vegetation, is not at all surprising. While the reefs, especially those of tropical littoral zones, offer a lot of varying microlandscapes to their inhabitants, a fact which in the long run must be ideal for speciation, just the opposite is the case on the level sea bottom. A sandy bottom has roughly the same appearance and structure whether in cold or in warm seas. It consists of the same type of substratum, thus being a habitat for morphologically the same types of invertebrates whenever it occurs. This, of course, is also the case for mixed and for muddy bottoms. Quantitative samples give further proof of this; a square meter of level bottom at the same depths and of the same substratum will comprise roughly the same number of invertebrate species whether in the arctic or in the tropics.

The only fundamental difference between an arctic and a tropical level bottom of the same type is the temperature of the sea in which it occurs. Therefore, it seems most urgent to clear up finally the old question as to whether or not marine invertebrates are adapted to the temperatures at which they normally occur. Comparisons of the metabolic rate of marine lamellibranchs within the same or closely related genera seem to show that a total adaptation or a close

approach to such an adaptation actually takes place (Spärck, 1936b; Thorson, 1936). A series of experiments by Fox (1937, 1939a, 1939b) and by Wingfield (1939), which apparently contradicted such temperature adaptations, have recently been corrected by Thorson (1952) and Berg (1953) who show convincingly that an adaptation actually takes place if body weight and age are taken into account. A recent paper by Scholander, *et al.* (1953), based upon several groups of marine poikilotherms, supports the same view: that a total or nearly total adaptation to temperature has taken place.[1]

Since this rule of adaptation seems to hold good for the different species of the same genus which replace each other in accordance with latitude and temperature, it will also hold good within a series of parallel level-bottom communities, because here again we have different species of the same genera replacing each other as the dominant animals. Furthermore, this means that, within a chain of parallel level-bottom communities, an arctic community will have roughly the same metabolic rate and rate of growth at 0° C as its boreal parallel at about 8° C, its mediterranean parallel at about 12° C, and its tropical parallels at still higher temperatures. In other words, the mutual competition among species, for instance within an arctic *Macoma* community, will be totally or nearly identical to the competition within the parallel *Macoma* communities from boreal, mediterranean, or tropical seas, and so on. The animals within the whole series of parallel level-bottom communities thus have to face the same problems in their struggle for life, whether the community inhabits high latitude seas or lives near the equator.

The practical value of this discovery to ecological workers is evident. A team of scientists studying parallel communities in different seas and latitudes, such as those of *Macoma*, *Amphiura*, or even *Venus*, might work in close collaboration, knowing that any observation on predation, mode of feeding, competition, or growth done on any coast will be a direct help in explaining similar phenomena in all parallel communities in other seas as well. However, the basis for such teamwork must be a thorough quantitative mapping of the level-bottom communities, especially along such coastal stretches where permanent biological stations are situated.

Having developed the concept of parallel level-bottom communities, it seems reasonable to inquire what this concept actually represents. Most modern ecologists agree in regarding the level-bottom communities as good *statistical* units. The question is: are they *ecological* units also and, if so, on what ecological relations do they depend? Are we concerned with a relation between the animals and the sub-

[1] See also Bullock, 1955.

stratum they inhabit or with a mutual relation between the animals within the community or with a combination of both?

In other words: How is a level-bottom community established?

The larval ecology may help to give us the answer. In arctic-antarctic coastal areas and in the deep sea, the level-bottom communities are composed of species with large yolky eggs and with a nonpelagic development. But in shallow water, which is where most of our bottom-dwelling food fish are concentrated, development by pelagic larvae prevails already in cold-temperate seas and is totally dominant in tropical areas (Thorson, 1950, 1952).

The pelagic larval life of most level-bottom species is hazardous. Fluctuations in temperature, food supply, and in the stock of enemies will decide which set of species is going to perish and which is going to survive until metamorphosis each season. The stock of surviving species varies much from year to year. Large numbers of a species in the plankton may reach the stage of metamorphosis one year but be totally lacking the next. And it is from these fluctuating sets of larval species that the level-bottom communities have to be recruited.

Vertical pump collections have shown that most species of planktonic larvae are restricted to rather thin horizontal strata of water, the vertical range of which is often only a few meters (Thorson, 1946). These larvae, being extremely sensitive to light, salinity, and temperature, stick to the stratum which most closely fulfills their varying needs. Larvae which, when ready to metamorphose, are photopositive and at the same time resistant to low salinity and varying temperature will crowd in the surface layers and will accordingly never get in contact with muddy bottoms in deep water. Contrary to this, larvae which are photonegative, preferring at the same time high salinity and a stable temperature, will crowd in the deeper layers and will never get in contact with a sandy bottom in quite shallow water.[2] Thus, out of the number of larval species which survive until metamorphosis and which qualitatively and quantitatively may vary enormously from year to year, only a selected and often reduced set of species will get in contact with each type of bottom, and each level-bottom area must be recruited from this narrow set of "existing probabilities."

Wilson (1937, 1948a, 1948b, 1952), in a series of remarkable experiments, has shown that the larvae of several polychaetes, when ready to metamorphose, will critically examine the bottom substratum

[2] Banse (1955) confirms the narrow horizontal stratification of planktonic larvae, which he, however, is inclined to explain more by the biological than by the physical qualities of the water masses.

to which they are exposed. If they find it attractive, they settle. If they find it less attractive or directly repellent, they will continue their pelagic life for days or even weeks. During such a prolonged larval life these larvae test the substratum at intervals as they are transported by the current directly over the bottom. Since such a transport at only half a knot, for instance, may carry the larvae about 170 km per week, their chance to find a favorable substratum seems fairly good.

Laboratory experiments confirming this have been made also on some archiannelids, some echinoderms, and on some other polychaetes. The question, whether similar reactions occur also within other groups of marine invertebrates and under *natural* conditions, has been answered positively by Erik Smidt (1951). In the Danish Waddensea the flood comes in from the North Sea at regular intervals, bringing with it a lot of "foreign" larvae ready to settle. During a thorough study of the microfauna of the Waddensea bottom throughout the year, Smidt did not find a single young bottom stage of any of the high-tide larvae mentioned above. In the microsamples examined, only such bottom invertebrates which also inhabit the Waddensea as adults were present as newly-settled young.

The so-called "gregariousness phenomenon" is known for such sessile hermaphroditic species as oysters, balanids, and *Spirorbis*, in which larvae are attracted by secretions or excretions of adults of their own species. This phenomenon will hardly occur among level-bottom invertebrates, since young of lamellibranchs, for instance, settling on a level bottom area among dense populations of adults of their own species, are directly known to lose out because they are unable to compete successfully with the adults.[3]

Thus, to all larvae of level-bottom invertebrates hitherto examined in the laboratory or in nature the bottom substratum seems to be the main factor that is responsible for their settling and, accordingly, for the qualitative composition of each level-bottom community.[4]

But is this rule valid for all types of settling larvae? What about the larvae of predators? Are they attracted by the substratum only or, perhaps preferably, by the presence of such prey animals as those on which their future life will be totally dependent? During their pelagic larval life, predators such as starfish, brittle stars, *Polynices*,

[3] See Knight-Jones (1951a, 1951b) and Knight-Jones and Stevenson (1950) on the gregariousness phenomenon. See also Thorson (1952: 291–292) for discussion of this subject.

[4] Exceptions to this rule may occur among those species of lamellibranchs which attach themselves by a byssus during their early life on the bottom and which bury in the substratum when they grow up.

etc. are dependent on and controlled by the same type of food as are the larvae of their prey, namely phytoplankton. This fact supports the view that even metamorphosing larvae of such predators have hardly any urgent need of animal food, since they may prolong their pelagic life after having tested a repellent bottom and since, during this prolongation, they will continue to feed on a vegetable diet. Furthermore, the ability of even adult *Asterias* to trace their prey by sensory activity is poor (Galtsoff and Loosanoff, 1939). It is known that some starfish do not hunt prey until at least three days after settling (Coe, 1912), while other starfish and some *Polynices* are known to settle earlier or at the same time as their prey (Mead, 1899; Orton and Fraser, 1930; Davis, 1925). These facts and several others seem to support the view that the settling of level-bottom predators is induced by a stimulus from the substratum and not by the prey which live on it and which will be hunted later on by the predators.

This discovery, that the bottom substratum seems to be the "master factor" which is responsible for the settling of nearly all pelagic larvae of level-bottom invertebrates and which accordingly is responsible for the qualitative composition of the level-bottom communities, is more valuable to marine ecologists than is immediately apparent. It must be remembered that in the open sea the uppermost layer of the level-bottom substratum tells us the story of recent sedimentation. This means roughly that a muddy bottom will normally be an indicator of calm water at somewhat greater depth, with a rather stable temperature and salinity; on the other hand, a bottom covered with coarse sand will normally result from turbulent water, found at rather shallow depths, with larger fluctuations in temperature and salinity. In this way the substratum will be responsible for the composition of the fauna and at the same time will act as a common denominator that tells us a good deal about the ecological conditions.

The fact that pelagic larvae of most predators are controlled by the same vegetable diet (phytoplankton) as are the larvae of their prey will also influence remarkably the balance between the number of predators and prey on the bottom; i.e., the balance between supply and demand. However, several predators have a longer life cycle than most of their prey; thus they are present in large numbers as adults when a new generation of prey settles down. In such cases it might be expected that dense populations of predators would totally "wipe out" all species of prey animals in a level-bottom community.

This might be expected especially in the case of the densely crowding and voracious brittle stars. For instance, the arms of *Ophiura* and

Amphiura on several mixed and muddy bottoms in all latitudes of our globe make so dense a network that every square centimeter of bottom is searched for prey every 24 hours. It seems surprising, therefore, that newly-settled prey animals, for instance the lamellibranchs, are able to survive this heavy predation during their first two months on the bottom when they are still tiny enough to be ingested by the brittle stars.

I hope that what I am going to say now will demonstrate the understanding of the balance within level-bottom communities which might be gained by 1) comparing a series of parallel communities, 2) focussing upon the biology of one of their dominant genera, and 3) studying carefully the biology of a species of this genus within one of the communities in order finally to generalize the results obtained for all of the parallels.

Let us focus upon the densely crowding *Amphiura* species which are known to dominate the *Amphiura* communities from the North Atlantic, the Black Sea, the Persian Gulf, Japan, New Zealand, etc., and let us see how they behave in the Danish Sound. Just at the time when larvae of prey animals settle in large quantities from the plankton, apparently running the danger of being quickly and totally eaten by the crowding brittle stars, these amphiurans enter a "passive period" of about two months during which they do not eat at all. During the first month they are loading up for spawning. Their swelling gonads completely fill out the disk, the stomach is reduced and its wall is even in histolysis. After spawning these stomach walls have to be built up again before the amphiurans can start their feeding activity; this process of building up will take about another month.[5] When the brittle stars have finally regained their appetite, most of the young prey animals which settled during the last two months have grown to such a size that they are too large to be ingested by the brittle stars. It may hardly be questioned that these passive periods among the amphiurans and the stimulating influence of the substratum on settling larvae are the two fundamental factors governing the species composition of all *Amphiura* communities.

Several other predators among marine invertebrates have similar passive periods which make it possible for their prey to survive their most critical age. We know that different starfish stop feeding during the month just before spawning only to be the more voracious afterwards (Galtsoff and Loosanoff, 1939). Even if such passive periods are significantly shorter than those of brittle stars (for instance, one month only), in the long run their presence may be of fundamental significance to the survival of prey populations.

[5] For similar observations in other ophiurans, see Glaser (1907) and Wintzell (1918).

As is well known, growth increase in newly-settled bottom invertebrates will often take place rapidly. A mussel, *Mytilus edulis*, about 0.35 mm long at settling, will grow during the following month to about 10 mm in length (Coe, 1945), which means that its content of organic matter expressed in nitrogen will increase about 2,000 times (Joergensen, 1952). Several other lamellibranchs and gastropods will grow at a similar rate, but even if we take into consideration the more slowly growing young of some species, we can certainly assume an average increase of organic matter of about 500 times for all prey animals during their first month on the level bottom. This means, furthermore, that a predator which has been passive for one month during the season of settling and rapid growth of its prey may obtain as many calories by swallowing a single prey animal as it might have obtained one month earlier by swallowing 500 prey animals of the same batch. Under natural conditions such passive periods often seem to occur during the right season for the initial rapid growth of their prey, and a passive period of one month's duration during the right season may reduce the waste of prey specimens to only 0.2 % of what would have been needed one month earlier, without reducing the caloric content ingested by the predator. In this way a lot of prey animals are saved from destruction.

To oyster-, cockle-, clam-, and scallop-farmers, who cultivate in their beds stocks or species imported from other localities and who suffer much when their cultures are attacked by starfish, it might be useful to find out the passive seasons of the starfish populations. They could then select stocks of lamellibranchs for their culture which would, at the prevailing temperatures, spend their first month on the bottom during these passive periods of their enemies.

Summarizing the main features of the marine level-bottom communities, we find that: the structure and composition of the substratum is the most essential single factor influencing the composition of the community; in the open sea the substratum is, in general, a common denominator for several other ecological factors; the passive periods among predators just at the time of settling and early growth of their prey animals guarantee that a species which has been attracted by the substratum and settled voluntarily on a level bottom is not likely to be fully exterminated; in most cases the final species composition of a level-bottom community is thus determined at the moment when the larvae settle there permanently.

I hope I have chosen the right examples to convince you that the ecological conditions prevailing in marine level-bottom communities are simple and well suited for research. The chains of parallel communities, comprising species of the same genera inhabiting the same

types of substratum in all latitudes, directly invite us to start a world-wide investigation.

We have come together here to open a new laboratory and to plan future research. Why not use this opportunity to start fruitful teamwork on marine level-bottom communities?

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CONSIDERATION OF MIDOCEAN FISH PRODUCTION AS RELATED TO OCEANIC CIRCULATORY SYSTEMS

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The sea receives most of the radiant energy reaching the earth simply because there is more sea than land, especially in middle and low latitudes where the angle of incidence is greatest and the loss by reflection is least. Therefore, so far as total absorption of energy is concerned, the sea's productive capacity should greatly exceed that of the land. Indeed Riley (1944) has estimated carbon production in the sea to be about 86% of the total carbon production on the earth, both land and sea combined. Steemann-Nielsen (1952), by a different method of measuring carbon fixation in sea water, arrived at a much lower rate of production, but even this lower rate would leave the production capacity of the sea equal to that of the land.

By contrast, man's total harvest of organic material from the sea is at least a whole order of magnitude lower than that from the land. Practically all of this harvest is taken from the margins of the sea. The oceanic part, beyond a depth of 1,000 m, comprises nearly 90% of the sea area, and from this major portion man's present harvest is trivial.

It is proposed in this essay to examine the means by which man may extract some of the organic wealth from this major pelagic fraction of the sea. Illustrative material will be drawn from what I believe to be the first attempt to focus scientific effort of nearly adequate scale directly on this problem. For this reason, it will be convenient to use a narrative arrangement, and consideration will be limited to the oceanic middle and low latitudes of the Pacific Ocean and to the extraction of organic materials in the form of edible fish.

The scientific effort to which I refer is the activity of the Pacific Oceanic Fishery Investigations of the U. S. Fish and Wildlife Service, headquartered at Honolulu, Hawaii, and assigned to the task of investigating the high-seas fishery potential of the tropical and subtropical mid-Pacific waters. Three vessels, the HUGH M. SMITH, the JOHN R. MANNING, and the HENRY O'MALLEY, later replaced

by the CHARLES H. GILBERT, have now been operating on this program for 4½ years. The first-named has performed most of the oceanographic work while the other two have been engaged primarily in assaying the fish stocks through systematic fishing surveys.

Hawaii is located near the middle of the sluggish westward-moving North Equatorial Current. In this Current a warm upper layer overlies the colder water and forms a two-layer system of persistent stability. Typically, the warm surface layer is at least 100 m thick and has a phosphate concentration of 0.2 to 0.4 μg-at/l; zooplankton is sparse, averaging 20 ml displacement volume per 1,000 m^3 in the upper 200 m layer as sampled with a 0.65 mm meshed net. Such waters are obviously an unpromising environment for supporting large fish stocks unless there exist situations where the vertical stability of the two-layer system is modified so that the underlying nutrient-rich water is mixed into the lighted upper layer in greater proportion than is prevalent in the North Equatorial Current.

It was our hypothesis that in situations of such modified vertical stability there would be chemical enrichment of the euphotic zone, supporting increased biological productivity and large stocks of fish, just as is true wherever there are mechanisms for enrichment in the marginal seas flanking the continents.

Three such situations have received varying degrees of attention during the last four years: the equatorial divergence-convergence system south of Hawaii; the boundary between the North Equatorial Current and the North Pacific Drift north of Hawaii; and the perturbations within the North Equatorial Current as it flows past or through the Hawaiian Islands group. Each of these involves a different physical mechanism for energy transformation into vertical motion: upwelling in the first situation; current interface shearing in the second; and eddies and wakes in the third. All of these situations appear effective for producing important concentrations of fish. Our knowledge regarding the first is now considerable. It is only fragmentary for the second and third. Even in the first situation there remain large areas of ignorance in our understanding of the physical and biological phenomena involved.

At the outset of our investigation, upwelling along the Equator, as recorded in the data of the CARNEGIE expedition of 1928–1929 and as interpreted by Sverdrup, *et al.* (1942; 710), appeared to be the most promising large-scale mechanism for producing concentrations of fish. Here, some kinds of fish should be abundant if our hypothesis was valid. It was hoped that these would include the two tropical and subtropical species of tuna that are of greatest commercial importance, namely the yellowfin tuna, *Neothunnus*

macropterus Temminck and Schlegel, and the skipjack, *Katwuwonus pelamis* Linnaeus.

Accordingly we undertook an intensive study of the equatorial current system. It has now been under way for four years. In this period we have made 16 hydrographic traverses of the current system at various meridians from Long. 140° to 180° W. On all of these traverses, bathythermograph casts were added between stations, usually at 10-mile intervals, and about 30 additional BT sections were made on fishing voyages. These extend our knowledge of the detailed temperature field down to 900 feet as far east as Long. 110° W. Several of the hydrographic traverses extended to Lat. 24° N and one to 14° S. However, the rectangle between Long. 140° and 180° W and Lat. 12° N and 10° S has been the most thoroughly explored. On many of the traverses, zooplankton hauls were made with a conventional meter-net, hauled obliquely from the surface to 200 m, with the flow through the net measured by a current meter.

These observations proved that there was indeed an upward movement of water at the Equator (Cromwell, 1951) and that it was accompanied by a greater concentration of zooplankton (King and Demond, 1953) in the vicinity of the Equator than in adjacent higher latitudes.

Despite the consistent evidence of chemical enrichment accompanied by zooplankton abundance, extensive fishing trials made by the three principal methods used in the California commercial tuna fishery (live-bait chumming, purse-seining, and trolling) proved disappointing. Except for small areas in the immediate vicinity of the few low-latitude islands in the region, tuna were not caught in the zonal band of enrichment.

Faced with this contradiction, we employed a fourth fishing method heretofore used only by the Japanese. Known as the "longline," this consists of a mainline suspended from surface buoys at intervals and provided with baited hooks hung by dropper lines at intervals along the mainline. This gear is constructed in units, known as "baskets," which are joined end to end and which may extend many miles. In our survey fishing, 12 miles of line are set, with the buoy-line lengths and the slack in the mainline so adjusted that the hooks fish at various levels from 100 to 400 feet, or for all practical purposes, throughout the entire warm layer and down partly into the transition layer.

This method of fishing proved to be satisfactory for sampling the larger pelagic carnivores and gave results consistent with the oceanographic findings. It has now been employed to delineate the distribution of tunas over a rectangle of about 4,000,000 square miles

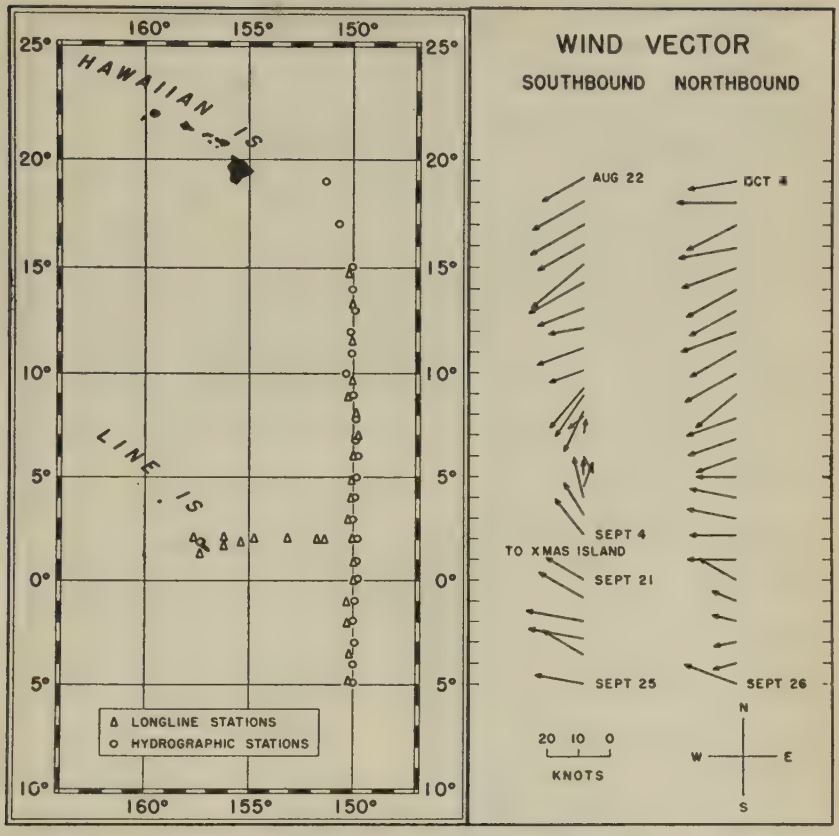

Figure 1. Hydrographic and fishing stations occupied by the HUGH M. SMITH from August 22 to September 26, 1951 (left) and the average direction and force of the wind at intervals along her track (right).

from Lat. 10° N to 5° S and from Long. 110° to 180° W. Further, the efficiency of this gear, first thought to be too low for use in the American economic structure, has proved sufficiently high to support American commercial operations when such gear is used in the areas of the greatest concentration of tuna as delineated by our surveys.

The findings with this sampling instrument in relation to the equatorial current system may be illustrated by an example along Long. 150° W. During her Cruise 11 in August and September 1951, the HUGH M. SMITH combined hydrographic, plankton, and longline operations along the track shown in Fig. 1. On the southbound traverse the winds were nearly typical for the region: northeast trade

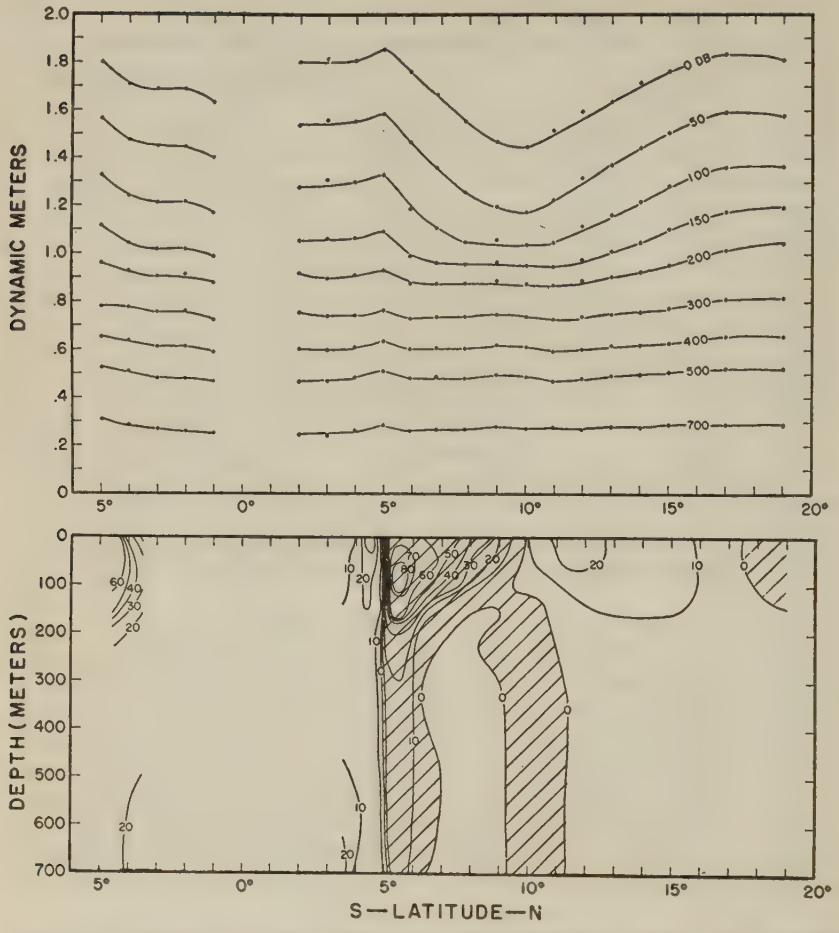

Figure 2. Dynamic topography (upper) and field of horizontal velocity (lower) inferred from geostrophic analysis of hydrographic data taken along the 150th W meridian by the Hugh M. Smith, August 22 to September 25, 1951. The unshaded portion represents flow to the west, the shaded portion flow to the east.

winds in the northerly portion of the traverse, southeast trade winds in the southerly portion, with light and variable winds between Lat. 5° and 9° N in a doldrum belt that separates the two trade wind systems. On the northbound traverse a month later, the doldrum belt was not in evidence, easterlies prevailed in the south, and the transition to northeast trade winds at 5° N was immediate. Geostrophic analysis (Fig. 2) of data from the northbound traverse

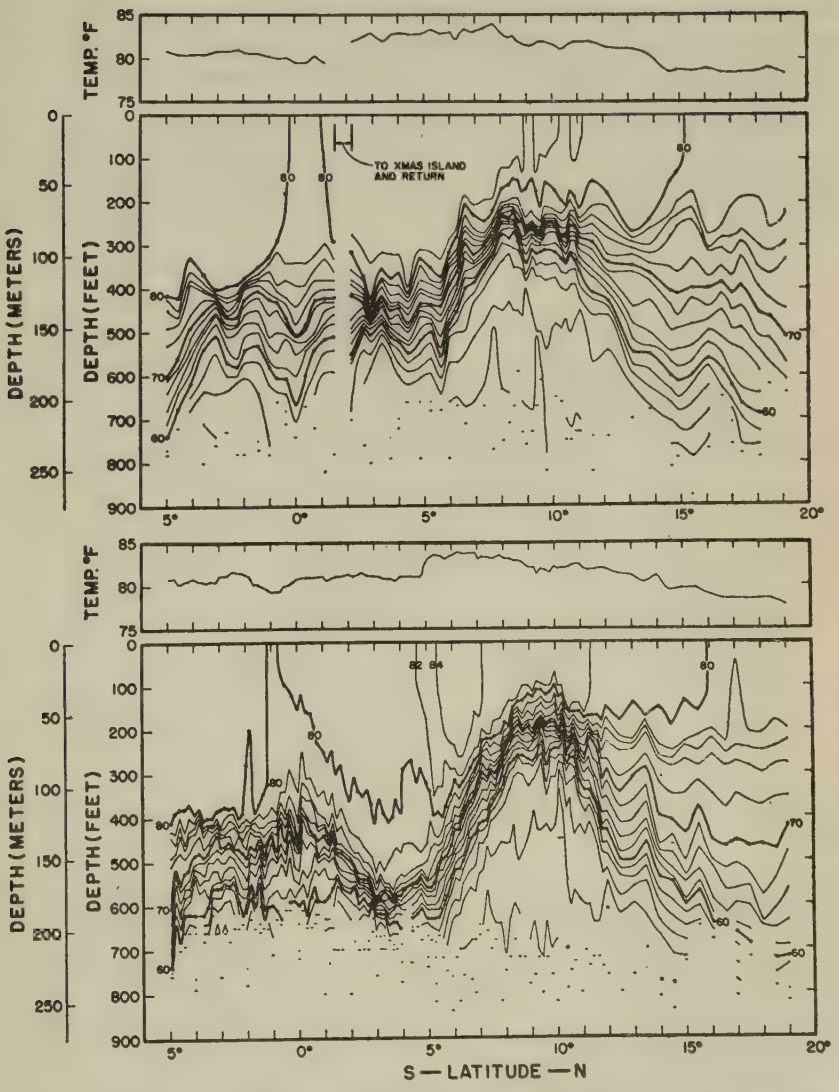

Figure 3. Vertical fields of temperature as measured by bathythermograph on traverses along the 150th W meridian: August 22 to September 25, 1951, southbound (upper) and September 26 to October 4, 1951, northbound (lower).

places the Countercurrent in its usual position between Lat. 5° and 9° N with a strong eastward flow of 80 cm/sec at its core. The drift of longline (Fig. 7) confirms this interpretation and also betrays the strong easterly undercurrent at the Equator, recently described by Cromwell, *et al.* (1954).

The transverse circulation is suggested by the distribution of properties in cross-section. Temperature (Fig. 3), density (Fig. 4), and inorganic phosphate (Fig. 5) show a weakening of stability at the Equator. The isopleths for 80°F, for σ_t23.0, for 0.6 μg-at PO_4-P/l

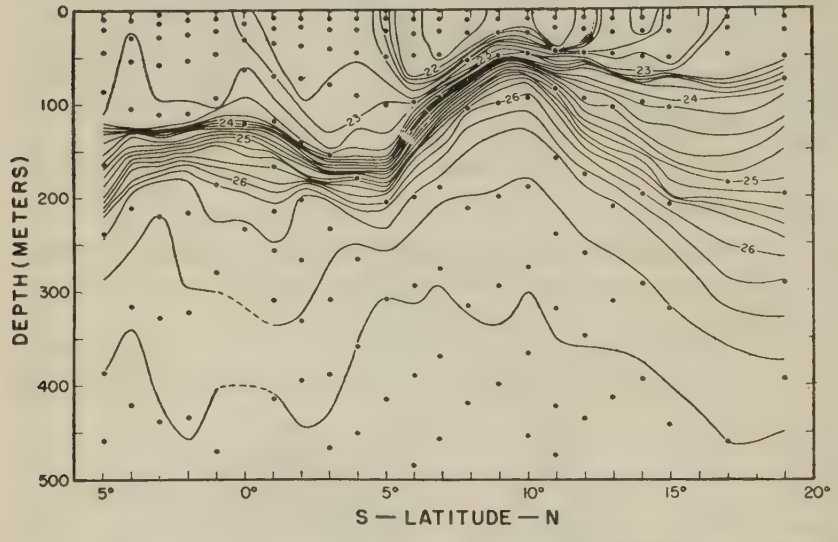

Figure 4. Vertical field of density along 150th W meridian, September 26 to October 4, 1951.

and for 4.5 ml 0_2/l ascend nearly vertically to the surface at or near the Equator, indicating relatively freer vertical exchange through the upper part of the water column at the Equator than elsewhere along the traverse. The isopleths immediately below those mentioned bend upward. This suggests that the exchange involves at least part of the underlying transition layer. In the deeper layers they dip downward under the Equator. Whether this is indicative of still deeper exchange is problematical.

It is not my intention here to elucidate fully the dynamics of this transverse equatorial circulation. As already suggested, there are features of it which are only partially understood and others are not understood at all. However, Cromwell (1953) has offered a model,

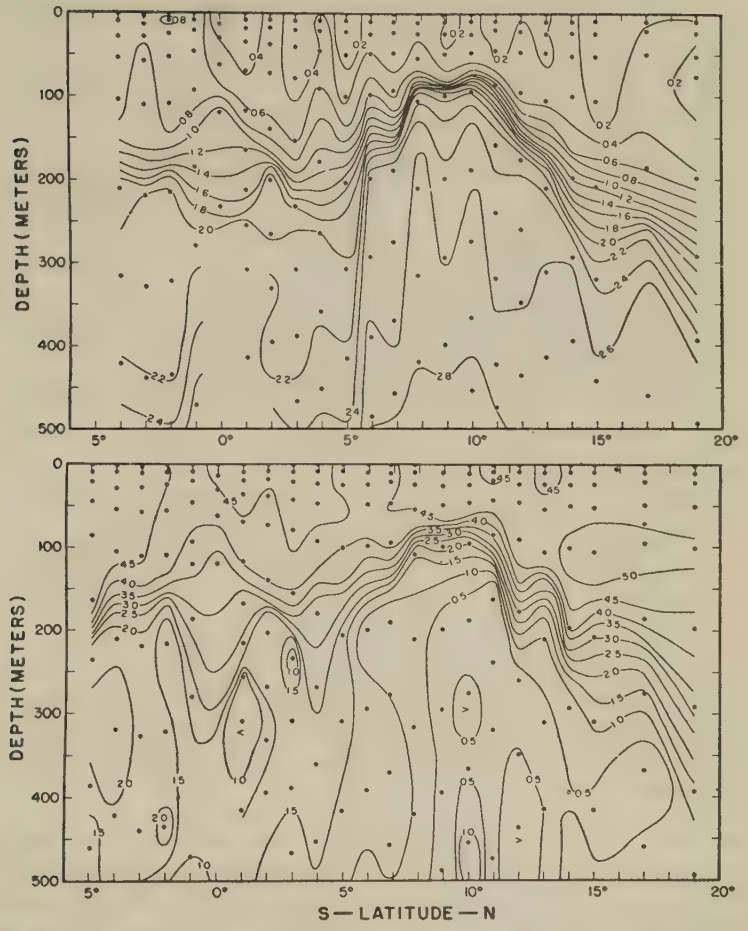

Figure 5. Inorganic phosphate in microgram atoms per liter (upper) and oxygen in milliliters per liter (lower) along the 150th W meridian, September 26 to October 4, 1951.

based on isentropic principles, which relates the transverse circulation to the winds and accounts for the major features we have observed in the distribution of properties in cross-section (Cromwell, 1954; Austin, 1954). We have reproduced in Fig. 6 the diagram of the transverse circulation as published by Cromwell (1953: Fig. 7). Briefly, the stress of easterly winds impels the surface water in a westerly direction at the Equator, but the Coriolis force north and south of the Equator tends to deflect the surface water poleward away from the Equator, where deeper water rises toward the surface.

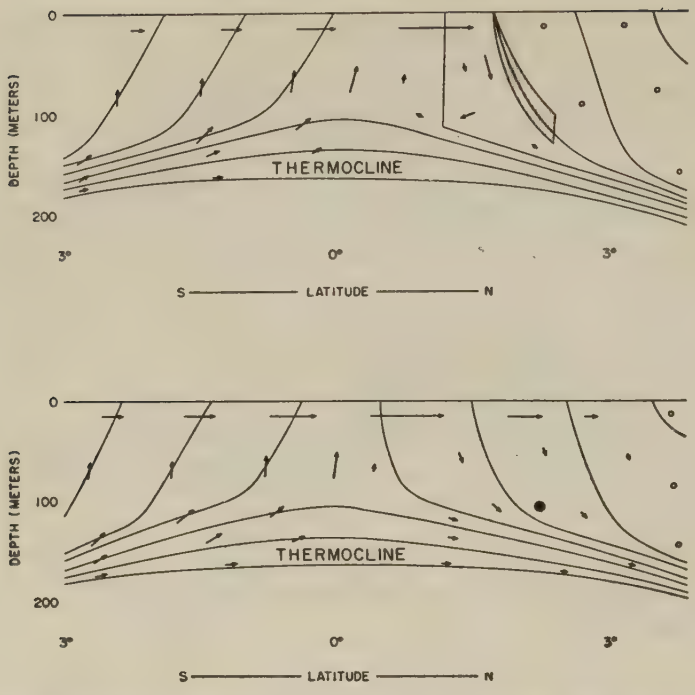

Figure 6. Schematic representation of circulation in the meridional plane near the Equator during a southeast wind when a front is present (upper) and when no front is present (lower). Lines represent surfaces of constant density. Arrows have the character of streamlines, not trajectories, and their shafts indicate roughly the relative magnitudes of flow. After Cromwell (1953: fig. 7).

Asymmetry is introduced when there are northerly or southerly components in the easterly wind. For instance, according to Cromwell, southeast trade winds displace the center of the upwelling to the south of the Equator. This appears to have been the condition during Cruise 11 of the Hugh M. Smith.

North of the Equator the upwelled water probably sinks, forming a zone of convergence between the center of the upwelling and the southern edge of the Countercurrent. Fronts are sometimes present in this convergence zone. The sinking water may return southward at deeper levels, thus completing a cell, or it may continue northward, diving under the Countercurrent. In the latter event, associated movement in other directions would be required by the principle of continuity.

When the zooplankton and fish catches along Long. 150° W are compared with the temperature section (Fig. 7), it is seen that the

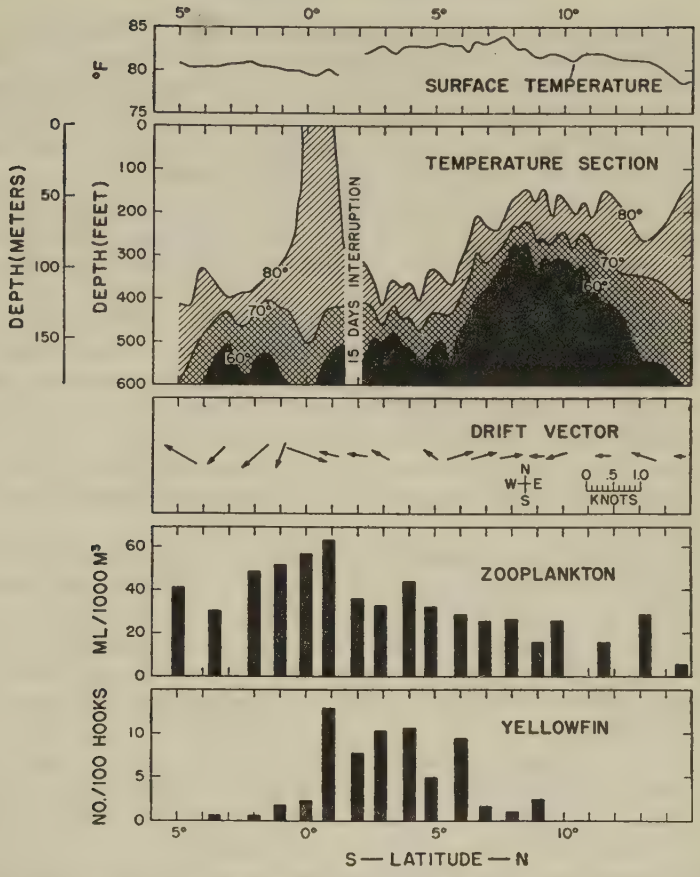

Figure 7. Surface temperature, vertical temperature field, longline drift, zooplankton abundance in the upper 200 m, and the yellowfin tuna catch along Long. 150° W, August 22 to September 25, 1951.

plankton concentrations are high in the vicinity of the Equator and that the tuna concentration is high in the zone of convergence. The displacement of the tuna northward from the plankton maximum is not unexpected since at least one trophic level (small fishes, crustacea, and squids) intervenes in the food chain.[1]

True upwelling seems to be absent at the northern boundary of the Countercurrent, though the transition layer here ascends so near the

[1] A speculative discussion of this displacement and also of the lack of a comparable tuna concentration south of the Equator was given at the 8th Pacific Science Congress (Sette, in press).

surface that a brief[2] invasion of the doldrum belt by high winds might stir the deeper, cooler, richer waters into the surface layer.[3] This evidently does not occur regularly enough to establish a trophic succession culminating with large fish, for we have not found a zone of yellowfin tuna concentration in or along the northern boundary of the Countercurrent, nor indeed in any part of the Countercurrent.

However, in the convergent zone area between the Equator and the Countercurrent, or sometimes in a band centering at the Equator, we have never failed to find a zone of much better longline tuna fishing than at adjacent higher latitudes. The quantities of catch and the positions of the zone in the north-south direction have varied considerably,[4] probably in response to accelerations and decelerations as well as to the swaying north and south of the current system. Although our observations have not had sufficient continuity in time and space to elucidate these variations, it remains quite clear that the divergence-convergence features of the transverse equatorial circulation provide the basic support for a persistent concentrated stock of yellowfin tuna.

The yellowfin tuna sampled by our survey are the older, larger individuals of the species, ranging from 60 to 180 lbs. and averaging about 100 lbs. The younger members of the species are nearly absent from our longline catches. So also are the skipjack. In our equatorial plankton hauls, however, the yellowfin tuna larvae occur regularly, and the skipjack larvae outnumber the yellowfin larvae by about three to one (Fig. 8). Obviously, equatorial waters must harbor young yellowfin in greater quantities than older ones (to allow for mortality), and in addition they must support still greater numbers of skipjack. A truly enormous oceanic fishery may be envisioned when the full potentials of this area are realized.

The east-west extent of the equatorial stock is limited only by the two continents, which at the Equator lie 10,000 miles apart. To be sure, this entire extent does not enjoy the high concentration we have found south of Hawaii. This concentration seems to be the consequence of the unequal stress of the easterly winds along the Equator. According to Cromwell's model, the upwelling should occur along

[2] The probably short-lived invasion by moderate northeast winds during the northbound traverse of the Hugh M. Smith apparently was without discernible effect on the vertical distribution of phosphate.

[3] This may have happened sometime prior to July 1952, for in that month unusually high plankton catches were taken in and immediately north of the Countercurrent along Long. 140° W. This is the only instance of such catches among the many zooplankton sections we have made across the Countercurrent.

[4] Some examples are given by Murphy and Shomura (1953).

the entire extent of the Equator lying under easterly winds, but it should be strongest and the enrichment of surface waters should be greatest where the winds are strongest and most sustained. According to Pilot Charts, the average wind force is maximal at about Long. 120° W. Therefore, the greatest enrichment should occur on the average at about the 120th meridian. If the two phenomena, maximal enrichment and maximal tuna concentration, are in cause and effect relationship to each other, then the westward flow of the water concurrently with the passage of the biota through at least three

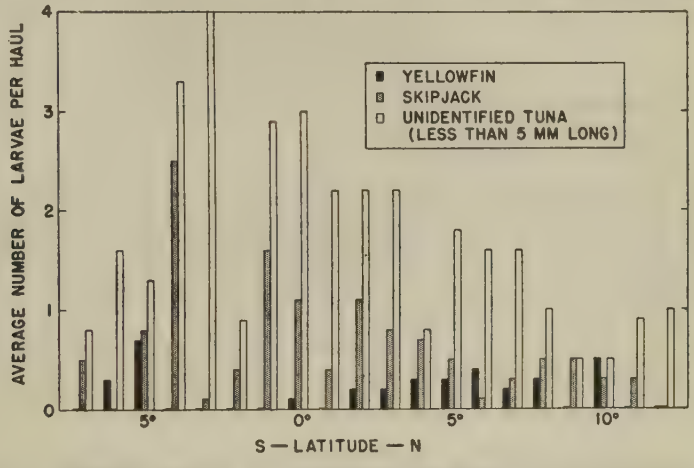

Figure 8. Average numbers of tuna larvae taken per oblique tow of 30 minutes duration through the upper 200 m layer with a one-meter tow-net having 0.65 mm mesh apertures. The samples were collected along various longitudes from 140° to 180° W and in various months during the period from June 1950 to July 1952. Larvae under 5 mm in length were not identified as to species.

trophic levels (phytoplankton, zooplankton, and smaller nekton) should have rates in reasonable relation to the distance involved. A parcel of water travelling westward from Long. 120° to 150° W at the rate of ½ to 1 knot (an estimate based on drift of longline in the convergence zone just north of the Equator) would be carried 1,800 miles to the zone of best tuna fishing in about 75 to 150 days. Whether or not this is a reasonable period of time for the three trophic levels of development is a speculative question. It does not appear to be fantastic, however, and it is the most cogent explanation I can offer for the special concentration of tuna at Long. 150° to 170° W (Fig. 9).

This concentration of tuna directly south of Hawaii is now undergoing development as a commercial tuna fishery resource by American fishermen. The lesser concentrations of yellowfin tuna lying west

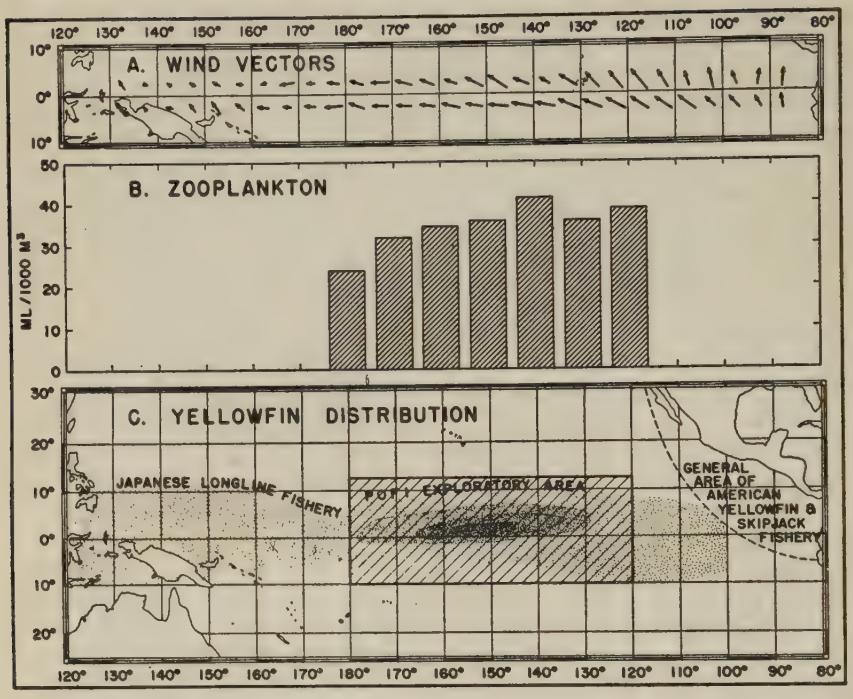

Figure 9. Wind, plankton, and tuna distribution in the equatorial zone. *A.* Resultant wind vectors were computed as the weighted means of frequency and speed from the several compass points for 5-degree squares bordering the Equator as published in the U. S. Hydrographic Office Pilot Chart for the North Pacific for August 1952; the arrows point in the direction of travel and their shafts are propertional to the weighted mean speed. *B.* Mean plankton volumes at several longitudes were computed as the mean of all hauls taken between Lat. 5° S and 10° N at or near the given longitude. *C.* Distribution of yellowfin tuna in the mid-Pacific equatorial region is estimated from longline catches. The light stippling in the POFI area represents less than three yellowfin tuna per hundred hooks, the heaviest represents more than nine per hundred hooks, and the intermediate stippling represents intermediate catching rates. For orientation as to location and area, the extensive band of light stippling represents the area of the Japanese prewar longline fishery; the dashed outline shows the area of the American west coast live bait and purse seine fishery for yellowfin and skipjack tuna. Since preparation of the above chart, a line of fishing stations along Long. 110° W has proved the continuity of the band of yellowfin tuna across the "unexplored area."

of the 180th meridian are already commercially useable in the Japanese economy. The lesser concentration to the east of Long. 150° W should ultimately be useable in the American economy when oceanographic advances make the finding of local concentrations within the convergence zone more sure and when technological advances make the fishing methods more efficient. It is perhaps not overly

optimistic to say that a vast oceanic fishery resource is well on the way toward full utilization.

This initial success of our hypothesis as a working tool for discovering fishery potentials in the oceanic two-layer system encouraged us to extend this type of approach in research to the second situation, the shear zone between the North Equatorial Current and the North Pacific Drift. In this more northerly latitude a cool-water tuna, the albacore, *Germo alalunga* (Bonnaterre), may be expected. Indeed, the Japanese have already developed a fishery for this species in the western Pacific between Lat. 30° and 40° N and between their home islands and the longitude of Midway Island some 2,500 miles eastward.

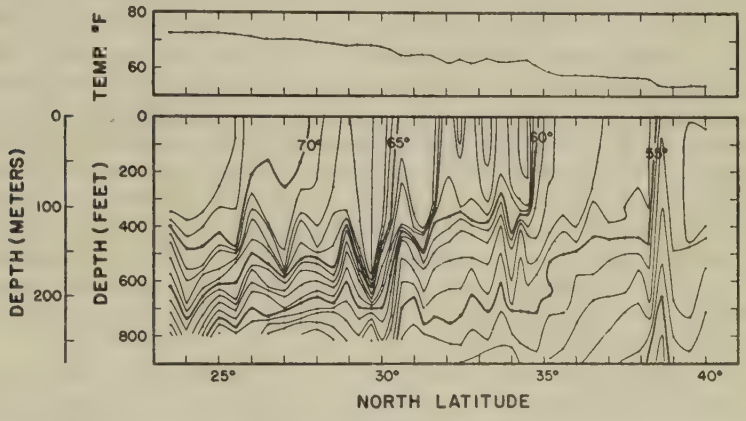

Figure 10. The vertical field of temperature as drawn from uncorrected bathythermograms along Long. 160° W in January 1954.

Uda (1953) has attributed this fishery to a feature he designates as the Subtropical Convergence.

Our first reconnaissance north of Hawaii in January through March 1954 crossed the North Equatorial Current into the North Pacific Drift along 10 meridional lines between Long. 140° and 165° W. The data from these crossings are not yet processed.[5] A preliminary temperature section (Fig. 10), drawn from uncorrected bathythermograph recordings, suggests a broad zone of complex thermal structure with most pronounced transitional features at Lat. 34° N. In several instances the bunching of isotherms in their vertical ascent from the

[5] The fishery research vessel N. B. SCOFIELD of the California Department of Fish and Game, in cooperation with the Scripps Institution of Oceanooraphy, made four more meridional sections between 140° W. longitude and the American west coast.

transition layer to the sea surface may result from eddy fragmentation in the shear zone. Their significance to vertical exchange must await further study. Between Lat. 30° and 35° N the longline fishing gave evidence of abundant fish life, including bigeye tuna, *Parathunnus sibi* Temminck and Schlegel, a species presently not considered a good canning variety, sharks, lancet fish (*Alepisaurus* sp.), and miscellaneous others including what is probably a new species of pelagic sting ray. At 34° on this section an outstanding catch of albacore was made. It seems quite likely, therefore, that we have here a general locality where the second type of vertical circulation provides the potentials of a fishery.

The third mechanism, which concerns vertical circulation of deeper waters into the euphotic zone within the North Equatorial Current system, should occur where islands interrupt the current's flow. This is suggested by the existence of a small but productive skipjack fishery in Hawaii. After vainly searching for enriching mechanisms connected with perturbations in the current flow past these islands, we recognized last summer the presence of relatively large-scale and semipermanent eddies downstream from the Hawaiian Islands. Curiously enough, these were recorded in the very first oceanographic observations taken by the Hugh M. Smith in the course of her shakedown trip in December 1949. They were first recognized however by GEK observations taken last summer. Both convergent and divergent types of eddies are formed (Fig. 11).

Near the boundary of one of the divergent eddies and far beyond the range of the present small local Hawaiian skipjack fishery, systematic scouting operations recorded many more skipjack schools than were sighted in the near-shore grounds or in neighboring offshore waters (Royce and Otsu, MS). This concentration was found on two occasions. Both times it was near the western edge of the most pronounced divergent eddy. This suggests that the eddy system has within it a train of physical and biological events of significance to the support of the fish. Also, the finding of these concentrations well outside the range of the present fishery suggests further that the productivity of this middle portion of the North Equatorial Current has potentials far beyond those suggested by the existing small local fishery.

Thus it appears that there are three situations in oceanic waters where the two-layer system is sufficiently modified to permit the vertical exchange necessary for supporting commercial fish stocks. Others may become apparent as our knowledge increases.

In comparison with the fisheries of marginal seas, the utilization of such oceanic fishery resources affords special opportunity for

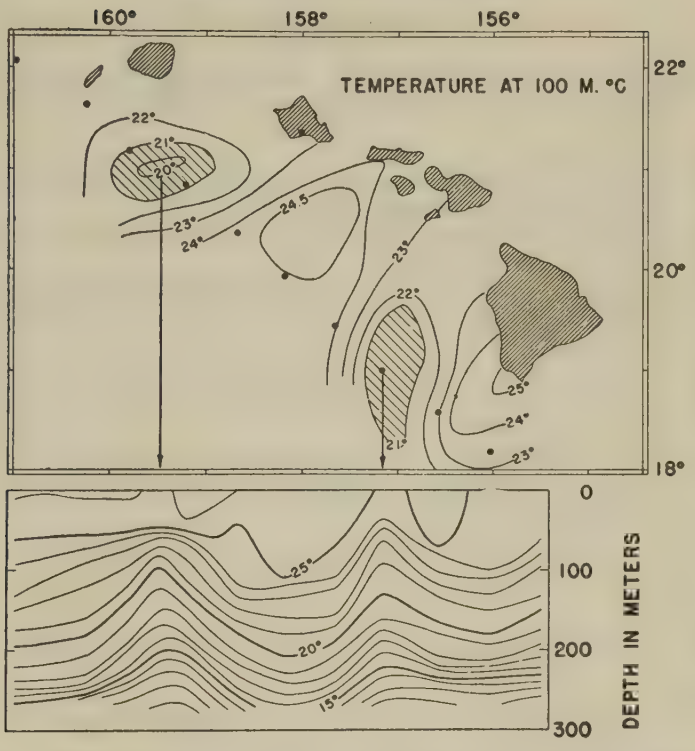

Figure 11. Temperature at the 100-m level (upper) and in a vertical section passing through the black dots of the upper diagram (lower) southeast of the Hawaiian Archipelago in August 1954.

oceanographic research to be of signal service. Unlike the fisheries of marginal seas, which were developed essentially by following topographic features to their seaward limits, the development of oceanic fisheries requires launching out beyond topographical landmarks literally into the deep blue. Success in locating fish resources in the deep blue does not reward the hit or miss method. Past ventures in the hands of so-called practical fishermen have proved this for the oceanic Pacific. At least four such ventures, incurring millions of dollars of expenditure, took place during the five years preceding the commencement of our work. None was successful. Fishermen's lore, evolved in the topographical setting of marginal seas, is not effective for guidance in oceanic fishing. Oceanographic research, on the other hand, promises to be effective in providing the needed guidance.

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~ *GEOLOGY AND GEOPHYSICS IN OCEANOGRAPHY*

SEISMIC MEASUREMENTS IN OCEAN BASINS[1]

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This paper will include a discussion of seismic effects and measurements in ocean basins as determined from natural earthquakes and from explosions. Earthquake seismology has been related to problems of the oceans since its beginning. In fact, establishment of the international network of seismograph stations, suggested by John Milne in 1897, was due in large part to the correlation of submarine cable breaks with oceanic earthquakes. Much of the great destruction of life and property caused by earthquakes at the borders of oceans results from the great sea waves or tsunamis occasionally generated by them. Recently it has become possible to draw quantitative information about the structure of the sea floor and its underlying rock layers from study of the dispersion of earthquake surface waves along oceanic paths.

On the other hand, explosion seismology began its growth about 1925 with knowledge of the "refracted wave" which had been accumulated in the study of nearby earthquakes; within ten years the first applications of it to problems of geological structure in the open ocean were underway (Ewing, *et al.*, 1937). Through the application of these two techniques, the major features of the geological structure of all oceans are now known.

The objectives of seismic measurements in ocean basins include measurement of the thickness and physical properties of the sedimentary layer, of the layer of silicic crustal rocks, and of the properties of the ultrabasic substratum which forms the upper part of the mantle of the earth. These structural elements of the ocean floor are to be compared with the corresponding ones for continents and for the zone of transition at the continental margins. Data on these structural elements are prerequisites for conclusions about the origin and evolution of continents and other major surface features of the earth, about

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principal mechanisms of deep sea sedimentation, age of the oceans, etc.

Our approach to this problem began 20 years ago with the adaptation of explosion seismology, as developed for oil prospecting, to investigations of the thickness of sedimentary layers in the ocean. Before long the goal of the work was near at hand, for we had not only excellent results at several stations but an excellent technique by which we could rapidly produce additional data on any occasion where two ocean-going ships became available. The physical problem had proved to be less difficult than we had expected, and it was possible to investigate the crust and the mantle in addition to the sedimentary layer.

At that time the possibility of obtaining the services of a team of two ocean-going ships as well as resources for making even a reconnaissance survey of oceans of the world seemed very remote. We believed that, for the immediate future, our seismic measurements would be confined to the area within about 600 miles of Bermuda and that it was of paramount importance to discover some new method by which, with the resources at our disposal, we could learn to what extent the results found by detailed study of the Bermuda area were characteristic of the oceans of the world.

We believed that knowledge of surface waves from earthquakes contributed a relatively neglected source of detailed information about the topmost layers of the earth's crust and that careful study of such data might show that the results from the area around Bermuda were indeed applicable to all oceans. The widespread distribution of epicenters in oceanic and coastal areas together with the extensive files of seismograms from island and coastal stations permit study of surface wave propagation along many great circle paths in every ocean. Thus we undertook a study of one part of earthquake seismology, treating it almost as a branch of underwater sound and attempting to use it as a tool to answer the specific question: Is all oceanic structure the same as that in the western North Atlantic? We now have earthquake data from the North and South Atlantic, the Pacific and the Indian oceans and explosion data from the Atlantic and Pacific, with a little from the Indian Ocean.

Crustal structure for these areas, if we exclude obviously anomalous features such as margins, mountain systems, trenches and island arcs, is as follows. An unconsolidated sedimentary layer, with thickness less than 1 km, covers 5 km of basaltic rock similar to that found near the bottom of the continental crust. The crust is underlain by ultrabasic mantle rock whose properties cannot differ greatly from those found at about 35 km depth beneath the continents.

There are some indications of the occasional presence of a thin layer of consolidated sediments or volcanics beneath the unconsolidated sediments. In contrast, the typical continental crust, composed of a layer about 35 km thick, consists of properties intermediate between those of granite and basalt but tending toward basalt at the bottom. Underneath is the ultrabasic mantle rock, probably dunite or peridotite. These results are compatible with the fundamental geodetic fact that the international gravity formula, established primarily from gravity surveys on continents, expresses the value of gravity at the surface of the oceans almost equally well. The obvious deficit of mass in the water layer must therefore be compensated by an excess of mass in the underlying rocks. It is therefore possible to use the seismically determined layer thickness and demonstrate that the densities required for balance are those of the rock types expected from geological studies.

With the major structural elements of ocean basins now understood, the next step should be the search for small differences between various oceanic provinces, the detailing of critical anomalous areas including continental margins, mountain ranges, island arcs, etc. Before sedimentation in the deep ocean is fully understood, details of layering within the sediments must be mapped. Careful examination of the upper mantle must be made to test whether or not the continents have evolved by differentiation from the subjacent mantle.

There are also several puzzling features on oceanic seismograms as well as a number of questions about behavior of surface waves at continental margins which remain to be solved.

Continental margins and anomalous areas usually occur in long narrow belts and include deep sea trenches and submarine mountain chains. Although they are the most significant regions from the standpoint of general tectonic processes in the crust, their structure is almost unknown. The principal methods which can be used for exploration of them are seismic refractions and gravity observations. Seismic studies are usually difficult because of thick sediments, rough topography, and rapid lateral variations of crustal structure. In order to measure properly a section across a continental margin by seismic refractions, it is necessary to orient each individual profile along the strike and to space these profiles at intervals of a few miles along the section. This requires much more time than that which has been available in the past when emphasis was placed on broad reconnaissance coverage. This also requires that the section be situated where a linear structural segment about 60 miles long is available and where some means of precise positioning, particularly for the receiving ship, exists. It is highly probable that explosive charges considerably in

excess of 300 lbs. will be required as the profile locations are moved toward the land. Great advantage can be gained by combining gravity and seismic surveys along each section, as has been demonstrated recently by Worzel and Shurbet (1955). The problem of topographic corrections is severe; generally they can be only partially solved at best. The receiving ship must be either anchored or returned periodically to a fixed position, and the sound velocity to be used in allowing for depth changes under both vessels will generally be uncertain. The typical parts of the mid-Atlantic Ridge, for instance, are very rough. It is doubtful if high precision delineation of crustal structure can ever be achieved here.

Increased use of high quality reflection shooting offers great hope in some marginal areas, particularly where the 60-mile segment demanded by the refraction method is unavailable. For example, as a small volcanic island is approached, the behavior of the Mohorovičić discontinuity may be investigated by carrying a reflection profile toward the island from the undisturbed ocean basin, with a tie to good refraction measurements as a starting point.

The great contrast between continental and oceanic crustal thickness establishes the permanence of ocean basins. The thin carpet of sediments, therefore, contains a record extending far into the past. The contribution of both earthquake and explosion seismology has been great in that it has proved that this layer is thin and that at least the greater part of its sediments is unconsolidated. It is now desirable to determine the fine structure within the sediments. By combining high quality reflection studies and refraction measurements in which particular care is taken to record, at close intervals, the various ground wave arrivals for shot distances up to 10 miles, velocity gradients and layering in the sediments have already been found. To obtain even more detailed information it may be necessary to return to the original method of making short refraction profiles with shots and detectors on the sea floor (Ewing, *et al.*, 1946). Even if world-wide correlations of sedimentary layering are found, it may be necessary to wait for the first borehole to interpret these indications of major events in the history of the earth.

It is already clear that there is no great difference in the composition of the upper mantle under oceans and under continents. It is very important to the study of crustal evolution to determine whether or not any systematic difference, however small, is present. An obvious procedure is to make more and better refraction profiles, on continents and in oceans, thereby gradually improving the reliability of the average values. Fortunately, two newly discovered earthquake phases provide a means of comparing continental and oceanic mantle

velocities; they are detectable to distances of at least 10,000 km, consist of compressional and shear waves respectively, and propagate in a channel of the upper mantle. These phases, discovered independently by Caloi (1953) and the authors (Press and Ewing, 1955), automatically yield average values, provided suitably located epicenters and seismograph stations are available.

The Sofar channel (Ewing, *et al.*, 1948), discovered during World War II, provides a mechanism for the transmission of sound waves from a small bomb to great distances in the ocean. The position of the bomb can be determined with high accuracy provided three receiving stations are available, leading to many potential applications for position finding in oceanographic research. "Acoustic shadows" and "topographic echoes," both caused by islands or sea mounts which reach near to the surface, have been used as a charting system for certain topographic features. An obvious extension of this work is the use of the T phase (Tolstoy and Ewing, 1950), a Sofar channel wave generated by earthquakes or submarine volcanic explosions. These phases can be of great importance in locating active submarine volcanoes and in some cases they can be of some help in earthquake epicenter location.

The exploration of ocean basins by means of earthquake seismology has been greatly retarded by lack of observations from within the ocean basins. Great dependence has been placed until now on seismograms from coastal stations. At best these records are difficult to interpret because many of the waves to be studied are stopped completely, or scattered, or transformed, or refracted at the continental margin. It is obvious that well equipped island observatories could greatly increase the usefulness of earthquake seismology as a tool for geological investigation of the oceans.

Even with the improvement and simplification in surface wave trains afforded by island observatories, the island itself produces great distortion of the wave pattern when the wavelength is of the order of the dimensions of the island or less. Apparently the best way to overcome this difficulty is to place seismograph stations on the floor of the ocean in areas remote from crustal anomalies.

The material discussed in this paper includes the work of many authors. For references and a more detailed discussion of many of the points, the reader is referred to a symposium on "The Crust of the Earth" (Geological Society of America, 1955).

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THE OCEANIC CRUST

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In the last half dozen years the nature of the crust under the oceans has been determined by seismic refraction work (Ewing, Worzel, Raitt, Hill, Hersey, Gaskell, and others). The Mohorovičić discontinuity is near 10 km in depth, and above this there are approximately 4 km of rock having the properties of basalt which are overlain by sediments of perhaps ¾ km thickness. These results have been confirmed by surface wave dispersion studies of earthquake wave paths crossing oceanic areas (Wilson, Baykal, Ewing, Press, Jardetzky, Officer, Evernden). All of this is a truly remarkable achievement, for it changes drastically all preceding ideas as to the nature of the crust under oceans. The unexpectedly simple character of this crust lends itself readily to geologic analysis. It may also shed some light on the origin and evolution of the earth's crust as a whole, a subject which has been, until the present, one of almost entirely uncontrolled speculation. We have known from the work of geodesists that the crust of the earth closely approaches isostatic equilibrium. So it is now profitable to make a petrologic balance between the relatively simple oceanic crust and the more complex continental crust. Though perhaps not generally recognized, samples of all of the rock types from the oceanic crust and from the underlying mantle are available, though the authenticity of some could be debated. In terms of geologic structure and history, I propose to discuss rather informally here the consequences which the oceanic crust would require.

The Nature of the Crust. The meaning of the term "crust" has various connotations to various people. In one sense it may connote the rocks above the Mohorovičić discontinuity, thereby referring those rocks below the discontinuity to the mantle. Or it may connote a stronger layer of rock above a more mobile subcrustal zone, the asthenosphere. In this paper I will use the term in the first sense because this gives a definite lithology as well as a definite lower margin as fixed by a marked seismic discontinuity. The other definitions, based on physical behaviour, particularly in the sense of a stronger

layer overlying a weaker one, have advantages from the structural point of view, but generally they leave thickness in doubt. In using "crust" in the sense I prefer, it must be borne in mind that a thin crust is not necessarily a weak one or a thick crust a strong one. This would confuse the two types of definitions. A thin crust, in my sense, might act as a structural unit with the upper portion of the mantle and be stronger or weaker than some other sector as the properties of the rocks and physical conditions would dictate.

It has been common practice to subdivide the crust into *sial* and *sima.* These terms refer to generalized compositions, *sial* being those rocks rich in Si and Al and *sima* those rich in Si and Mg. In turn these have been taken to be synonymous with *granitic* and *basaltic. Granitic* has been used in such instances in a broad sense, including rock types from diorite to granite, and of course the granitic crust also included various schists, gneisses, greenstones, etc. such as are generally found in old Pre-Cambrian terranes. *Basaltic* has been used in a composition sense rather than as an indication of grain size, texture, or lava flow origin. It might have appeared more consistent with granitic to have called it gabbroic, but historically it was introduced at a time when many accepted Daly's hypothesis of a vitreous basaltic substratum, an hypothesis generally considered to be obsolete today. Peridotitic material below the sima has been referred to as ultrasima by some whereas it may be lumped in the term sima by others. All of the above terms are more or less inextricably associated with one or more hypotheses so that it becomes difficult to use them without suggesting to the reader something more than the layer's composition. Sial, however, retains a certain usefulness and is not unduly involved in extraneous hypothesis.

Before considering the oceanic crust it would be well to outline briefly the status of the continental crust with which it must be in balance. Until recently, seismologists and geologists were strongly predisposed toward giving the continental crust a layered character. Postulates of an upper, intermediate, and lower layer were common in most discussions by seismologists. Today opinion seems to be moving away from this concept, though Gutenberg still favors a two-layered arrangement. At any rate, there is no very marked discontinuity until the Mohorovičić is reached. On geological grounds it seems unlikely that there could be layers representing distinct rock types in the continental crust; rather it is a complex mixture much the same in vertical section as that portrayed in horizontal plan in an area of Archean terrane. It has been repeatedly deformed in the course of time so that much of it should have steeply dipping gneissic structure. The rock types throughout would resemble those of the

old Pre-Cambrian as seen at the surface (granitic intrusions, schists, greenstones, etc.) but with an increasing proportion of the more mafic constituents downwards. The latter would be a result of repeated partial fusion in the deeper portions and of intrusion of the resulting liquids into the higher portions. The concentration of the more refractory constituents downwards and of less refractory upwards would result in bulk properties such that seismic velocities would increase with depth. In general, the more refractory groups of minerals give the higher seismic velocities.

The continental crust as envisioned above would be heterogeneous on a small scale (50 km) but homogeneous on a larger scale (1000 km). The Mohorovičić discontinuity is a uniquely sharp boundary seismologically but it could be irregular on a small scale.

The transition from continental crust with its base near 35 km to oceanic crust less than one fourth as thick takes place in a horizontal distance of perhaps 100 km. This would mean that the Mohorovičić has a slope of perhaps 15° downward from sea toward land. While the exact nature of the transition is not known, the recent work by Worzel on the gravity data and by others of the Lamont and Woods Hole groups on the seismic data should clarify the situation for the Atlantic coast of North America in the near future.

The average depth of the oceans is 3.8 km, but this is a rather meaningless figure for present purposes, since it averages the depths over the continental slope, deep sea floor and various ridges and seamounts. What is significant to the present discussion is the depth of the deep sea floor where it is uncomplicated by the above mentioned features. Over much of the oceans, great stretches of fairly level floor are found near 3000 fathoms, or approximately 5½ km. On the floor are unconsolidated oceanic sediments of variable thickness, but 0.7 km would be a good average figure (see Brilliant and Ewing, 1954, and Ewing and Press, 1952). Below is a layer generally considered to be basaltic in composition for which an average thickness of 4 km will be accepted here. This lies on the Mohorovičić discontinuity roughly 10 km below sea level. Below the discontinuity, rocks of peridotitic composition are found. Gaskell (1954) has summarized the velocities and layer thicknesses from seismic refraction work up to 1953, and a number of more recent profiles may be found in Ewing, Sutton and Officer (1954).

Evidence of the Character of Rocks in the Oceanic Column. The unconsolidated sediments found on the ocean floor have been described from many cores and dredgings, so no further consideration will be given to them here. Skipping for the moment a consideration of what lies between the sediments and the Mohorovičić, let us consider

the rocks below the discontinuity, because these are also known with a fairly high degree of certainty. These rocks are peridotites, consisting approximately of 4 parts magnesian olivine, 1 part enstatite, 1 part chrome disopide and 1 or 2% picotite. Ross, *et al.* (1954) have described more than a dozen rocks of this type which have been brought to the surface as foreign blocks in basaltic volcanoes. All samples have the same four mineral phases, and Ross, *et al.* have shown that chemically the minerals are practically identical from all of the localities studied. Their samples come from three continents and from oceanic islands. There can be little question that the samples do represent the material from beneath the Mohorovičić, and it is interesting to note that *the composition is the same below the oceans and below the continents.*

Table I gives the bulk chemical analysis of one of these peridotite xenoliths from an extinct volcano near Ludlow, California and of the peridotite from St. Paul's Rock in the Atlantic Ocean off Brazil (see Tilley, 1947). Note that the two analyses are practically identical. St. Paul's Rock represents an actual outcrop of the material

TABLE I. Analysis of Peridotites from below the Mohorovičić

	St. Paul's Rock Peridotite (a)	*Ludlow California "Olivine Nodule"* (b)
SiO_2	43.97	44.35
Al_2O_3	2.89	2.97
Fe_2O_3	1.04	.67
FeO	6.89	7.59
MgO	41.11	40.80
CaO	2.35	2.55
Na_2O	.07	.20
K_2O	Nil	.01
H_2O+	.35	.06
H_2O-	.20	.03
TiO_2	.17	.14
P_2O_3	ND	.02
CO_2	ND	Nil
Cr_2O_3	.50	.41
MnO	.13	.13
NiO	.21	.31
CoO	trace	ND
CuO	Nil	ND
NaCl	.09	ND
	99.97	100.24

(a) Analyst H. G. C. Vincent (see Tilley, 1947).
(b) Analyst R. B. Ellestad, new analysis.

which has world-wide occurrence as xenoliths in basalt. The same four mineral phases are present in it. The only difference is that the enstatite has exsolution lamellae of diopside parallel to {100} whereas the enstatite in the xenoliths does not. This merely indicates that the St. Paul's Rock sample cooled slowly, permitting exsolution, whereas the peridotite xenoliths, which came rapidly from a hot environment at great depth where the temperature was too high for exsolution to occur, were quenched upon reaching the surface. St. Paul's Rock appears to be the western end of a great fault scarp paralleling the equator and extending eastward through the Romanche Trench (Fig. 1). This may be a fault zone similar to those described

Figure 1. Great fault zones on the ocean floor. Four large zones in Pacific, after Menard (1954). The Barracuda and St. Paul's-Romanche zones in the Atlantic are added by the writer, and Eardley (1954) suggested the Pacific Galapagos zone. A seaward extension of the Montana Lineament is also postulated. P = peridotite; S = serpentinized peridotite; localities are mentioned in text.

by Menard (1954) in the eastern Pacific. Duplaix and Cailleux (1952) have found small fragments of peridotitic material in the samples dredged from the floor of Romanche Trench. It appears that this fault has enough displacement to bring sub-Mohorovičić material to the surface at St. Paul's Rock.

One would expect from peridotite of the above character a seismic velocity for Vp near 8 km/sec, which is in good agreement with observed velocities from below the Mohorovičić at sea. Other rocks, perhaps eclogites or rocks comparable to stony meteorites, might produce similar velocities but no such xenoliths as are found in oceanic volcanoes. Continental volcanoes do have a variety of different types of xenoliths as well as peridotites which are identical to those from oceanic volcanoes. But in the case of continental volcanoes, samples

of any rock type occurring in the continental crust may be found as foreign inclusions. The conclusion cannot be avoided that the rock forming the xenoliths must be the material from below the Mohorovičić under the oceans, and it seems probable from negative evidence that it is the only rock type present. On the basis of what is known of this rock's physical properties, it could make up the entire mantle to the core. Not enough is known of its properties nor of the physical constants of the earth's interior to make the above statement any more than a presently possible hypothesis.

Consideration of material lying between the Mohorovičić and the unconsolidated sediments remains. Commonly this is considered to be largely basaltic in composition. The only direct evidence, aside from seismic velocities, to substantiate this belief are: the small angular gabbroic fragments found by Duplaix and Cailleux (1952) with the serpentinized peridotite fragments in the Romanche trough; and the gabbroic and basaltic fragments along with serpentines dredged from fault scarps on the mid-Atlantic Ridge by Ewing, *et al.* Inasmuch as the above fragments come from scarps which also expose the ultramafic rocks from below the Mohorovičić, it seems reasonable to suppose that the gabbroic and basaltic debris represent the immediately overlying material. The fact that basaltic cobbles and fragments are found at many places on the ocean floor is suggestive but not acceptable evidence, inasmuch as these no doubt come from basaltic volcanoes which have cut through the crust and which are not necessarily the same material that makes up the layer in the crust considered here.

The total thickness of sea floor sediments which have been determined is less by a factor of 3 to 5 than previous estimates based largely on rates of sedimentation (Kuenen, 1946). This naturally raises the question whether some consolidated sedimentary rocks having seismic velocities near basalt could be included in the layer described above as basalt. Cherts and massive limestones or dolomites could have such velocities and are not unlikely as deep sea sediments. Such rocks are not among those dredged from oceanic fault scarps, therefore evidence, so far as it goes, is against the postulate.

It is possible that some serpentinized peridotite such as that found on the mid-Atlantic Ridge is present in the "basaltic" layer. Seismic velocities in two samples of such material were measured in the laboratory for the writer by E. C. Bullard; measurement gave for Vp 5.7 and 6.3 km/sec at atmospheric pressure. These velocities might be 5 to 10% higher at pressures consistent with their depth within the crust.

Balance of Crustal Columns. In a recent paper the writer (1954) tried to estimate the pressures at 40 km under both oceanic and continental crustal columns. The thicknesses of various layers and of probable rock types included in them were estimated from seismic data and from petrological inference. Appropriate densities, with due allowance for compressibility and thermal expansion, were attributed to the layers. Inasmuch as there is a great deal of inference that goes into such estimates and since there is comparatively little experimental data at both high pressures and temperatures, the values given cannot be rigorously defended. Recently density

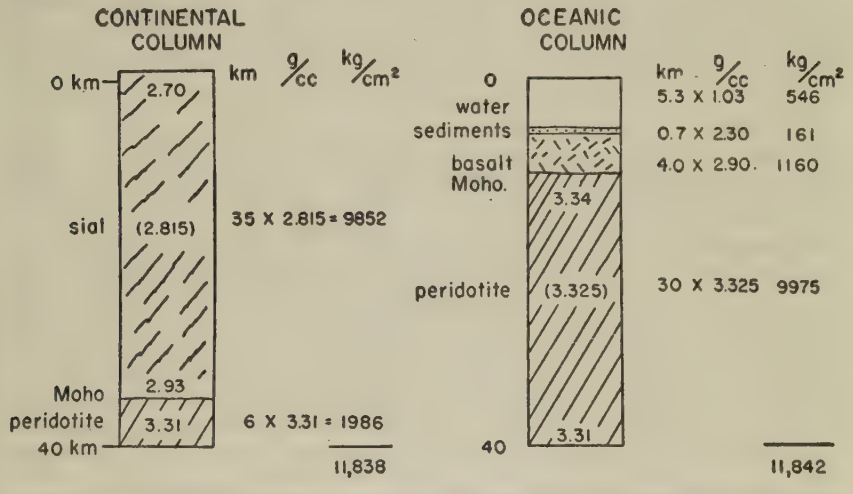

Figure 2. Comparison of layers, densities and masses of typical oceanic and continental columns.

determinations on samples of peridotites which are thought to come from below the Mohorovičić were made; they were found to be somewhat higher than those previously estimated. Four typical samples gave density values of 3.348 g/cc ± .005. A peridotite cited by Daly (1946) has a density of 3.35 g/cc at room temperature and pressure but 3.28 g/cc at 1500° C and 30,000 bars. From this the density of the peridotite under consideration here is estimated to be 3.31 g/cc at 40 km. It is important to note that in the uppermost part of the mantle the density should be decreasing slightly from the top of the mantle downwards. As the temperature gradient becomes less steep, this trend will be reversed. So little is known of the thermal gradient in depth that one cannot predict the position of this reversal.

In Fig. 2 the continental and oceanic columns down to a depth of

40 km are compared. With present seismic data and information on densities of the oceanic column, the pressure at 40 km cannot vary much from the 11.8×10^3 kg/cm^2 as given. The continental column is less certain. The average density of the continental crust probably lies between the limits 2.78 and 2.84. At 40 km these limiting densities would give pressures from 11.7 to 11.9×10^3 kg/cm^2. This indicates that the oceanic and continental columns are almost in balance, if not exactly in balance. Assuming an exact balance, the figure 2.815 g/cc is obtained for the density of the continental crust, the value used in Fig. 2.

The weight of the crustal column to 40 km is a convenient means of estimating either the amount by which a given column is out of equilibrium where the densities are known or of determining the density if it is assumed that equilibrium exists.

Serpentinization and Vertical Movements of the Ocean Floor. This topic was brought up in a paper by the writer (1954) and was discussed at some length in a second paper (1955). Therefore it will be dealt with only briefly here.

That volatile constituents contained in the interior of the earth are gradually leaking to the surface was specifically suggested by Kulp (1951). Rubey (1951) postulated that the oceans have been growing through geological time by some such process. If water were leaking from the interior, it would, upon crossing the 500° C isotherm, react with olivine and enstatite of the mantle peridotite to form serpentine as indicated by the experimental data of Bowen and Tuttle (1949). This reaction is exothermic and would involve a

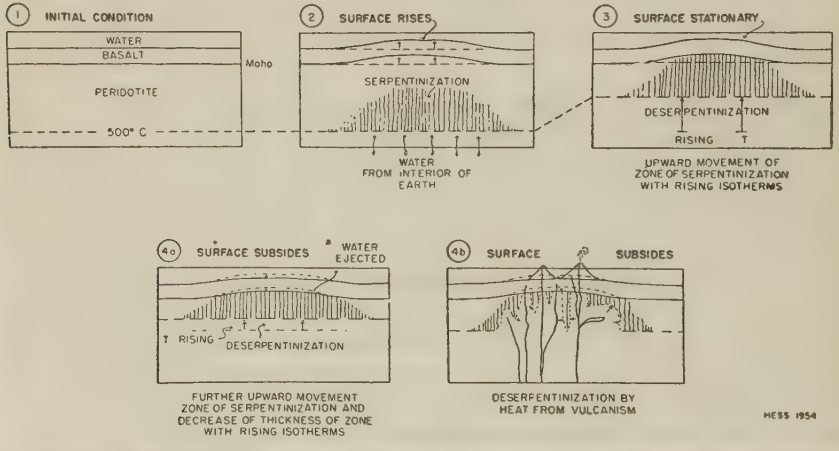

Figure 3. Diagrams to indicate how serpentinization and deserpentinization of mantle peridotite might cause vertical movements of sea floor.

volume increase of approximately 25%. The volume increase is approximately equal to the volume of the water added.[1] The operation of the process, indicated in Fig. 3, will not be elaborated on here.

Such features as the mid-Atlantic Ridge, mid-Indian Ocean Ridge and several oval-shaped plateaus more than a hundred kilometers across in the North Pacific and Indian Oceans might be attributed to water leakage from the interior of the earth under these areas and to consequent serpentinization with volume increase above the 500° C isotherm. Rise of the 500° C isotherm as a result of injection of basaltic magma, or perhaps convective overturn of the mantle, would cause deserpentinization and subsidence of the topography. In other words, the reaction is reversible. The submergence of the guyots of the mid-Pacific Mountain Range might be explained as a result of deserpentinization under a mid-Pacific ridge which once stood some thousands of feet higher than it does today.

The Andesite Line. The origin of andesites (the predominant extrusive of island arcs) and their intrusive equivalents, quartz diorites, requires reconsideration in light of the new information on the nature of the oceanic crust. Apparently the only magmas which cut through the crust of the ocean basins proper are basaltic, either alkali olivine basalt or tholeiite (Tilley, 1950). The temperature upon extrusion is near 1100° C for these basalts. Allowing for adiabatic expansion, their temperatures at their site of generation must have been somewhat higher. The depth at which such temperatures exist must be at least 60 km below the surface, or perhaps several times this depth. As suggested by Bowen (1928: 315–320), basaltic magma is presumably the result of *partial* fusion of the peridotitic mantle, material such as is given in the chemical analyses of Table I. It is unlikely that andesitic magma could be formed directly by partial fusion of peridotite, and its absence as a primary magma in oceanic areas seems to substantiate this view. Partial fusion of basaltic rock would be a chemically satisfactory source for andesitic magma. Basalt of the normal oceanic crust is at much too shallow a depth and hence at too low a temperature to suffer partial fusion. Apparently only in connection with island arc deformation is the basaltic layer downbulged to a sufficient depth to generate andesitic magma. Even in this case the downbulging must be somewhat greater than that which has been indicated thus far by seismic work over trenches. This, however, is a possibility, as will be pointed out in the succeeding section of this paper.

[1] At temperatures up to 700° C, water reacting with enstatite would produce talc with a 10% volume increase.

With a thick crust under island arcs, as was generally postulated before the advent of seismic work at sea, it was possible to derive andesite by partial fusion of this crust or by contamination of basaltic magma passing through such a crust. Consequently it was concluded that andesitic volcanism indicated that island arcs represented the seaward boundary of continental crustal conditions, i. e., the outer margins of continents. This concept is no longer tenable. An oceanic type of crust has been found between the arcs and the continents and inside such seas as the Caribbean. The andesite line does not represent the continental margin. Rather, andesite will be produced where deformation is sufficiently strong to bulge the basaltic layer down to sufficient depth to cause partial fusion. For some as yet unexplained reason, such deformation commonly takes place in the oceans not far from continental margins, but if it were to take place in midocean, a similar result could be expected.

Gravity Anomalies and Island Arcs. The belt of huge negative gravity anomalies found in island arcs, as explained by Vening Meinesz as well as by Umbgrove, Kuenen, Griggs, Hess, and others, resulted from downbuckling or downbulging of the upper granitic crust which was then considered to be about 25 km thick. The mass deficiency of the downbulged granitic material as compared to the supposed basaltic material on either side of it was sufficient to account for the size of the anomalies and for the shape of the anomaly curve. In island arcs the mass distribution proposed in the above hypothesis is now known to be incorrect, as pointed out by Ewing and Worzel (1954) and Worzel and Ewing (1954). Substitution of new values for densities and thickness of layers, as required by recent seismic data, results in a new picture for the cross-section through the negative anomaly belt over trenches. The dimensions are changed, but qualitatively the structure may remain much the same.

An excellent topographic and seismic survey was made over the Tonga Trench on the Capricorn Expedition of Scripps Oceanographic Institution. The seismic profile was reported on by Raitt, *et al.* (1954). It shows a normal basaltic layer about 5 km thick away from the Trench which thickens to an observed 10 km in a profile run along the bottom of the Trench. The depth of the basaltic root under the Trench, however, may actually be considerably greater, inasmuch as the measurement may represent the slant distance to the Mohorovičić sideways at an angle of perhaps 45°. The vertical depth to the Mohorovičić could be much greater, as shown in Fig. 4 and as suggested by the presence of andesitic volcanism in the island arc to the west. The critical point is that the basaltic layer does thicken and hence the structure is to be explained by lateral compression. Sedi-

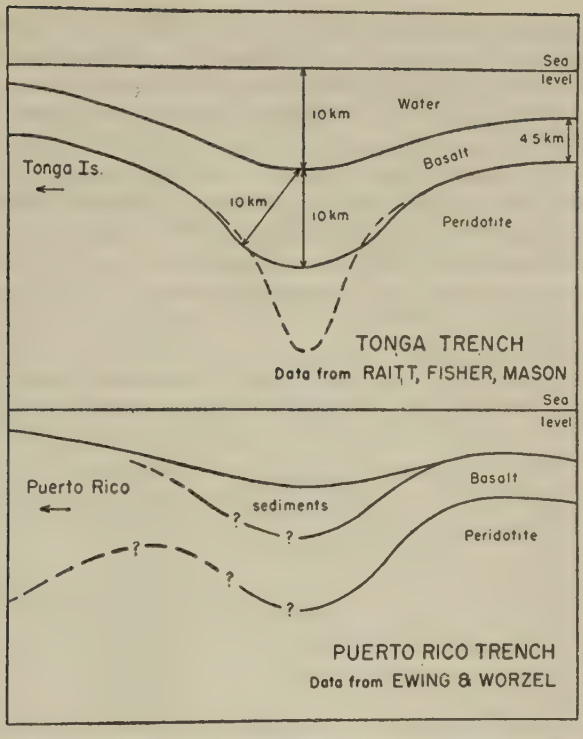

Figure 4. Sketches to indicate hypothetical structures of Tonga and Puerto Rico trenches. Vertical exaggeration about 2.5 ×. Data for construction of sketches taken from authors indicated, but writer takes responsibility for the interpretation as given.

ments are virtually absent from the floor of the Trench. Gravity data for this Trench, not obtained thus far, would be exceedingly interesting to have. However, the mass deficiency under the Trench profile may be estimated; it is at least as large as that under the Puerto Rico Trench, so one might expect similar or larger gravity anomalies (Fig. 5).

Worzel and Ewing (1954) have given excellent gravity data for the Puerto Rico Trench as well as a partial seismic profile. Their analysis of the mass deficiency based on the free air anomalies and computed from Vening Meinesz's tables is most enlightening. While the depth to the Mohorovičić could not be seismically determined under the Trench, an approximate thickness for the sedimentary fill was obtained so that the remaining thickness of the basaltic layer may be estimated from the gravity data. Fig. 4, which shows a profile through the Puerto Rico Trench based on the Worzel-Ewing

data, has been somewhat modified in form from their presentation. Their statement that the anomalies can be explained largely by the mass deficiency of the great thickness of sediment, while true in a sense, is misleading, for if the sediments were not present then the anomalies would be still larger. At least half of the free air anomaly results from the mass deficiency of the water at the top of the column. In the case of Tonga Trench one can estimate that the free air anomaly will be as large as or larger than that for the Puerto Rico Trench, though no sediments are present in the former.

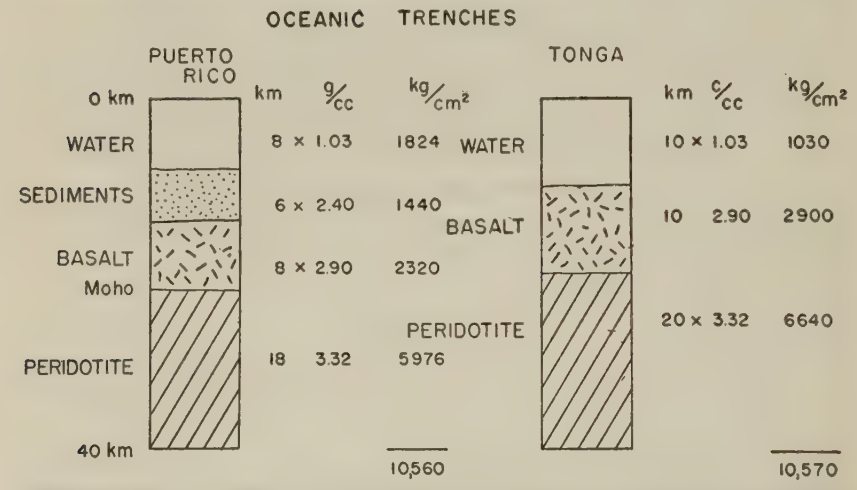

Figure 5. Mass distribution under trenches. Compared to normal oceanic column (Fig. 2), these show a mass deficiency equivalent to differences in pressure at base of columns equal to 1.3 $\times 10^3$ kg/cm².

Though free air anomalies are useful for a first approximation and for comparing trenches with one another, topography plays too large a part in the resulting numerical values. Inasmuch as the topography is usually well known, it should be eliminated so far as is possible in order to get information on the deeper structure. A type of modified Bouguer anomaly might be devised to do this. Using a datum plane of 3000 fathoms, the mass deficiencies of depressions below this datum or excesses above it could be computed by using a suitable density for the oceanic materials; thus the effects could be removed from the observed gravity value. Various isostatic anomalies in which the effect of topography and compensation are introduced are useful if not essential to understand the meaning of the gravity data. Objections have been made to isostatic anomalies at sea on the basis that the compensation element in them is purely fictitious.

Granted that this is so, particularly in the case of trenches, they are none-the-less useful in that they indicate how the real structure departs from the assumptions. For the area over a trench, the isostatic anomaly would be derived by removing the effect of mass deficiency of the topography and by compensating in depth with material of higher density. Geologically this would probably be impossible inasmuch as there is no rock type with a density higher than peridotite that could be placed in this position. Computation of the isostatic anomaly over the Puerto Rico Trench confirms this in showing that it is not compensated and hence is out of isostatic equilibrium, at least locally. If it is not compensated locally, other types of isostatic anomalies could be used to find out whether it is compensated regionally and over how large a radius.

The same conclusion of lack of isostatic equilibrium of the trenches could be reached from an analysis of the seismic data. Comparison of the normal oceanic column (Fig. 2) and trench columns (Fig. 5) shows a difference in pressure at the base of the columns amounting to more than 1×10^3 kg/cm^2. Considering the large horizontal area of the trenches, it is quite impossible that the crust could be this far out of isostatic equilibrium *unless a force were acting on it to keep it out of balance.* The value of approximately 1×10^3 kg/cm^2 is a measure of the vertical component of that force. This could mean a compressive force acting tangentially of at least three times this amount. If this force were not acting, then the trenches would rise to the equilibrium position. In the case of the Puerto Rico Trench it might be expected to rise from 4300 fathoms to form a ridge about 1250 fathoms below sea level. This would produce a topographic feature similar to the Barbados Ridge, but unlike the latter it would not then have a large negative isostatic anomaly above it. The Barbados Ridge must represent a great thickening of the sedimentary and basaltic layers so that it is still almost as much out of isostatic equilibrium as is the Trench. In essence it is developing a downbulge more nearly comparable in size and thickness to the original picture presented by Vening Meinesz. Presumably continued deformation and thickening will eventually produce from such a structure enough sialic material to support an alpine type of mountain range when the forces acting on it die out.

The structural interpretation of geophysical data often cannot be made in complex areas without reference to geological history. In the case of the Puerto Rico Trench, it was initiated as a structure at some time after the Paleocene. Previous to that time a trench probably existed trending ESE along the south side of Puerto Rico. Puerto Rico itself was on the crest of an island arc which probably

was concave toward the NE and which contained numerous active volcanoes along its length. This volcanism of Late Cretaceous and Paleocene built up a huge pile of lava, agglomerate and tuff which resulted in a greatly thickened crust under the area of the present island. The sediments and volcanics in the trench to the SW were strongly deformed, probably in the early part of Late Cretaceous, and were converted to schists, the extension of which are now seen to crop out in the Cordillera Central of the Dominican Republic and also in the southwestern corner of Puerto Rico. These geological circumstances strongly affect the interpretation of the southern half of the Worzel-Ewing profile. This information was not available to them when they presented their results.

Evolution of an Island Arc into an Alpine Mountain System. This concluding section is frankly speculative. It is advanced to direct attention to items which may give critical evidence on island arc structure and development.

The development of island arc structures into alpine-type mountain structures is a well substantiated hypothesis. The evolution is a much longer and more complex process than this writer (Hess, 1938) originally envisioned. The Tonga Trench may be pointed to as the first stage in development. Here, if the deforming forces were to die out, the Trench would disappear and a gentle ridge 500 to 1000 m high on the sea floor would take its place.[2] The thickened basaltic layer would then be floating in isostatic equilibrium. The Puerto Rico Trench has not gone so far in its evolution, but it has received a huge volume of sediments. Large islands are nearby to provide a source of sediments, and the bottom topography is such that turbidity currents which carry these sediments drain into the Trench. There is no evidence that the sediments in it have been deformed as yet. Inasmuch as andesitic volcanism occurs in the arc to the west of the Tonga Trench but not in relation to the Puerto Rican, one might postulate that the basaltic root under the Tonga Trench is deeper.

The next more advanced stage may be represented by the Barbados Ridge. It is still far out of isostatic equilibrium. A downward bulge, probably of sediments underlain by metamorphosed sediments and basaltic material, altogether perhaps 30 km thick, has developed, judging from isostatic anomalies. Active volcanism in the Lesser Antilles indicates a deep root. If the Barbados Ridge were relieved from compression, it would rise to form a low island chain with the

[2] The rise necessary to bring the weight of the crustal column back to 11.8×10^3 kg/cm^2 at 40 km depth assuming a minimum thickening of the basaltic layer (10 km).

rudiments of alpine structure. Probably it would not, if arrested at this stage, form a high mountain range. The islands of Hispaniola and Cuba probably represent just the situation speculatively proposed above for the Barbados Ridge development. To produce high mountains, much more crustal shortening and a much larger downbulge would be required. One may question whether island arcs starting on a true ocean floor will ever develop beyond the stage of a Cuba or Hispaniola in one mountain-building series of episodes. Many island arcs of the past which developed only to the early trench stage may today form rather inconspicuous topographic forms on the ocean floor.

With the above reasoning, one may ask how one would ever develop a mountain system such as the Himalayas. The clue to this seems to be given by following the Antillean chain southward and westward into the Cordillera along the northern coast of Venezuela. Where island arc-type of deformation impinges on the edge of the continent, the thin sialic crust may be downbulged in exactly the manner postulated formerly by Meinesz for the negative strip of island arcs. The great Alpine-Himalaya chain probably began along a mediterranean of oceanic character and transgressed in places on the border of a continent. The apparent termination of some continental mountain systems abruptly at the sea may mean that their extensions on the ocean floor are inconspicuous and are perhaps covered at present by sediment, as indicated in the preceding paragraph. This might explain why the early Paleozoic folded mountains seem to disappear at the coast of Newfoundland and reappear in northern Ireland and Scotland. Traces of this might be discovered by an increase in depth to the Mohorovičić along the line joining the two ends.

Finally it should be pointed out again, as the writer has done in the past, that island arc-alpine type of deformation rarely if ever takes place within continents proper which have a normal thickness of sialic crust. The forces may be the same but the reaction to them different. The best example of this type of reaction is the Rocky Mountains of the United States. It is comparatively rare for mountain building to occur deep within continents.

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RECENT AND ANCIENT SEDIMENTATION; TWO ASPECTS OF ONE SUBJECT

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Geology has thrived on the application of the maxim, the present is the key to the past. Investigation of the phenomena and active processes of the surrounding world has proved to supply the most trustworthy foundation for interpreting the features of ancient rocks and unravelling the geological history of the earth's crust. But one chapter of geology has formed an exception, that of ancient marine sediments. Considering the extent as well as the economic and paleontologic importance of these rocks, it is a huge chapter. In this domain geologists have attempted the opposite procedure, namely to deduce the submarine processes from the features of their consolidated rocks.

Deep-sea deposits, it is true, have received a certain amount of attention, but they are too scarce in the geological column to be of much use in the study of ancient sediments. In themselves they are of fundamental importance to the earth sciences, being closely tied up not only with geophysics, paleoclimatology, and geoeconomy but no less with dynamic, chemical and biological oceanography. But this is not the subject of the present paper, although there will be occasion to return to consideration of deep-sea deposits later on. The following remarks, it should be realized, apply mainly to the sea floor at depths less than 100 fathoms.

During the last two decades, and more especially since World War II, a change has come about. Many have begun to realize not only the painful void in our understanding of what is going on on the sea floor but our inability to interpret the mode of origin of the rocks that enclose the bulk of our fossils, sedimentary ores, and oil, deposits used as fertilizers, building stone, raw materials for chemical industries, etc. F. P. Shepard has been particularly active in showing how hopelessly inadequate are our ideas on the distribution, nature and origin of recent marine sediments, especially those present in shallow water.

Several institutions are now embarked on investigations of the shallow sea floor; oil companies, navies, and universities are providing

funds, with the result that knowledge is expanding rapidly. Just how rapidly will be realized by noting the increasing number of papers appearing on topics in the broad field of marine sedimentology. Almost all geological journals frequently contain papers of this kind.

It is not generally appreciated that there are two entirely different approaches to the study of shallow sea floors. The main objective can be a knowledge of the laws of sedimentation by ascertaining the properties of the sediments and of the environment. This kind of work will gradually teach us how to determine the type of environment that existed when ancient marine sediments were formed, and geologists will thus learn to unravel the paleogeography of the continents. In fact, the present sea floor will be used as a key to the past.

The opposite objective is to extend the geological map beyond the shore line out to the continental shelf. Thus data are sought which will tell us the history of a local area of the crust that happens to be covered by water at the present time. Whether the formations encountered are of marine origin or not is of no particular concern.

It is of some importance to realize this dual aspect of submarine geology, because techniques, teamwork, and suitable areas vary with the objective. For the study of sedimentation, areas of deposition must be sought. Physical, chemical, and biological oceanography of the environment has to be taken into account. Transport by waves and currents, source of mineral and biological components, solution and precipitation, diagenesis on the sea floor are of fundamental significance in these matters. Hence, a staff of specialists is required, and only a large oceanographic institution can tackle these problems successfully. Of course an individual, working on his own, can, with ingenuity, advance our knowledge as to certain aspects of sedimentation, but by and large marine sedimentology is a subject for teamwork. For most purposes simple grab samples will suffice, because bulk properties are the most important. But bottom photography and the complicated apparatus needed by the other oceanographers render a large ship necessary and hence require backing by ample resources.

On the other hand, the regional geologist, when venturing beyond the shore, will prefer areas of nondeposition or of thin sedimentary cover so that he can find the older formations at the surface or only a short distance below the obscuring sediment. He will require little equipment other than some heavy coring device or dredge, or he may use a self-contained diving outfit instead. Once he has secured his samples they need less deep-going investigation as to grain size, organic content, chemical composition, water content, etc. so long as he can recognize the formation they come from.

Thus an oil company considering the lease of an area on the sea floor will conduct field work in the latter sense. But if the research department of the same company seeks to learn more about the general principles of deposition they will sponsor the former type of investigation.

It is obvious that the two approaches noted above are interrelated. Luckily they are by no means exclusive of one another, and in practice both will often be included at the same time. Nevertheless it is important to bear in mind the basic differences between the two aspects of sea-floor study and to lay particular emphasis on one of the two.

An illustration of the combination is found in the study of stratification in cores of recent sediment; change in material is linked with a change in conditions, and the sedimentologist will be practicing regional historical geology when investigating the deeper strata of his core. Thus both approaches are combined in one study.

An instance of geological mapping of the sea floor is seen in the examination of the English Channel by investigators from Cambridge, Britain (King, 1950; Hill and King, 1952). They have taken samples of Mesozoic rock with a heavy corer, often below a thin cover of recent sand.

An example of work that is aimed purely at learning more about sedimentology is furnished by Stetson's well known investigations of the New England shelf and Gulf of Mexico terrace sediments (Stetson, 1953). Other instances could be given, such as the American Petroleum Institute Project for Gulf Coast sedimentation.

I will restrict myself to the recent reconnaissance of the Orinoco shelf area by a Dutch group. This project, sponsored by Shell Oil of the Hague, was carried out in 1952 and 1953. That particular region was chosen because it embraces within a limited area not only the protected Gulf of Paria but a large delta and an open oceanic shelf in the South Atlantic as well as one in the quiet Caribbean.

The report on the results obtained in the Gulf of Paria, which has just appeared (van Andel and Postma, 1954), deals with the current system during the dry season, when the Orinoco has little influence on the water circulation, and during the wet season, when the surface waters of the Gulf become brackish. In the wet season a vast amount of sea water is carried along by the fresh water that escapes into the Caribbean. This loss is compensated by a powerful incoming bottom current which strongly counteracts the spread of sediment away from the river mouths. In addition, throughout the year a clockwise eddy occupies the eastern half of the Gulf and greatly reduces sedimentation in its centre. Thus hydrography is used to elucidate sedimentological data.

All of the sand carried out to sea by the river is now deposited within a few miles of the coast. Powerful currents sweep the mud into the Gulf of Paria, but a large portion escapes again on the far side to the Caribbean. Hence, much of the Atlantic shelf off the Orinoco and Trinidad is nondepositional, whereas the shelf north of the investigated area receives a great amount of fine deposit.

The land surface which formed in the Gulf of Paria during low sea levels of the Ice Age now lies buried under fine mud, as determined by echo-sounder. The rate of sedimentation deduced from the estimated time of postglacial transgression is variable, ranging from zero at some spots with high current velocities to one foot per century elsewhere. After consolidation, this would still amount to about 10 centimeters per century.

The large distributary of the Orinoco that debouches in the Gulf of Paria at Pedernales appears to be young because the levees are poorly developed. The time of breakthrough can be estimated in the following manner. At various spots in the Gulf, the large grab sampler revealed beds of decaying shells which had lost their colour and had become brittle. They appeared to be fixed in position of growth and were covered in most spots by a few inches of mud. At present the floor of the Gulf is practically devoid of living pelecipods. This implies that the breakthrough at Pedernales brought in such quantities of fresh water during rainy seasons as to kill off the shell beds. C^{14} measurements on a few samples have given ages of roughly 700 years for these shells. Since that event the rate of sedimentation appears to have increased, especially near Pedernales.

The organic matter of the Gulf mud varies in nature according to location. The carbon-nitrogen ratio is higher close to the river than it is farther away where planktonic life probably provides a more important contribution. Space does not allow me to dwell on the many other significant findings of this cruise.

The contention that a single individual is still able to carry out valuable sedimentological research can be illustrated by pointing to the work on the tidal flats of Holland and elsewhere by Dr. van Straaten (1954) of Groningen. Tidal flats form an entirely different environment from the open sea floor or deep bays, and such studies require other techniques of investigation. Dr. van Straaten has spent several seasons on this work and has achieved important results. His investigation of Devonian rocks in the Ardennes demonstrates the usefulness of the experience gained on recent sediments for interpretation of ancient rocks. In many respects the Psammites du Condroz are almost identical with the tidal sand and mud flats along the North Sea coast. In both areas similar sand and mud rocks were laid

down; one finds the same current ripples and wind ripples, the same gullies, mud pebbles, worm tubes, and mud cracks; there are identical lamination structures due to tidal currents alternating with slack water and comparable distortion by burrowing organisms. Even the area covered is of the same order of magnitude. But in one respect there is a significant contrast in that current action in the present tidal flats is much more active than that in the Devonian environment. Tentatively van Straaten concluded that the Psammites du Condroz were laid down in lagoons similar to those bordering the Gulf Coast, but later he changed his opinion (personal communication).

As a final example of the close relation between the investigation of recent sedimentological phenomena and ancient rocks, I would like to choose the insight gradually gained in the action of turbidity currents. Twenty years ago Daly came out with his epoch-making hypothesis which suggested that the submarine canyons off the New England coast had been scoured out during the Ice Age by currents of turbid waters rendered heavy by a load of mud. The author's experiments were later duplicated by Bell, and the principle of turbidity flow was confirmed.

Soon after, Stetson emphasized the significance of this mechanism for transporting fine material to the deep sea. Again a few years later the writer used experimental approach to supply convincing evidence that such a current could carry vast quantities of not only mud but sand as well and that it should be able to roll pebbles for great distances, even out to the deep-sea floor.

Migliorini was the first to suggest that the emplacement of certain sands showing graded bedding might be attributed to the action of turbidity currents. In graded beds the grain size decreases from the bottom to the top of a granular deposit. Again by experiment the author was able to confirm this and ventured to suggest that deep-sea sands might owe their origin to the transporting power of turbidity currents. Shepard and Phleger, and more especially Ewing and his associates, demonstrated the graded bedding of many deep-sea sands, their shallow-water components, their wide extent, and the great individual thickness of some of these beds. The latter group of workers has brought together an imposing array of arguments to indicate the vast importance of turbidity flow for deep-sea transport and for smoothing the topography of the ocean floor in the Atlantic.

The next step was Heezen and Ewing's (1955) analysis of the cable breaks following two earthquakes. They contend that a turbidity current which was triggered off ran down the continental slope at a velocity of over 50 knots. This hypothesis fits the data so perfectly and the subsequent proof of a thick bed of graded silt on

top of normal deep-sea sediment in that area is so suggestive that there is not much room left for doubt. However, so staggering are both size and velocity of the assumed currents that many may still feel inclined to ask for further proof.

To round off this picture of turbidity currents the author would like to draw attention to results on graded bedding in ancient rocks of the graywacke type (Kuenen, 1953). It has now been demonstrated in a dozen formations of graded rocks of all ages that the graded beds are associated with features which indicate that they have been deposited by currents. These currents had a constant direction through many successive beds and over wide areas. Traced up-current, these beds make place first for nongraded rocks and finally for shallow-water deposits. This forms strong supporting evidence for the great importance of turbidity flow as a transporting and depositing agent in the oceans, both past and present.

In conclusion I should like to emphasize on the one hand that further insight in the sedimentology of the present oceans is of vast importance to geology and the earth sciences in general and on the other hand that marine sedimentology is a field from which a rich crop of results with fundamental significance can still be gathered.

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ON THE HISTORY OF THE OCEANS[1]

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I shall discuss two problems concerning the history of the ocean: (1) The volume of water on the earth's surface at different times in the past. (2) Former conditions in the water and in the overlying atmosphere compared with those existing today. Specifically, we may ask whether the waters were acid, or slightly alkaline as at present, and whether conditions were oxidizing or reducing, that is, whether or not free oxygen was present.

Much of the discussion is not original. It owes a good deal to the flashing insight of Harold Urey, who was among the first to apply the principles of chemistry to the problems of the early history of the earth and the solar system; to the skillful observations of Gerard Kuiper, who has described in quantitative detail the atmospheres of planets other than our own; and to the careful analysis of William Rubey, who has marshalled the facts of geology to throw light on the broad questions we are discussing.

The scarcity of the noble gases (neon, xenon and krypton) in the earth, compared with their cosmic abundance, shows that these gases were driven away from the earth at some time in its early history. Other gases of comparable molecular weights must therefore have escaped also if they were not held in some kind of chemical or physical combination (Brown, 1952). Thus, in this respect the atmosphere and the ocean must be secondary. The ocean waters did not condense from a primitive hot and heavy atmosphere but were squeezed out of the interior of the earth. But the rare gases probably escaped from the "protoplanet" before the earth achieved its present size and degree of compaction (Kuiper, 1952: 320–324). We may ask, then, did the squeezing out of the water and of the atmospheric gases take place mostly before the geological record began or have the atmosphere and the ocean slowly grown throughout geologic time?

During the past 50 years geologists have expressed diverse views on this question. Some advocate the doctrine of permanency, that

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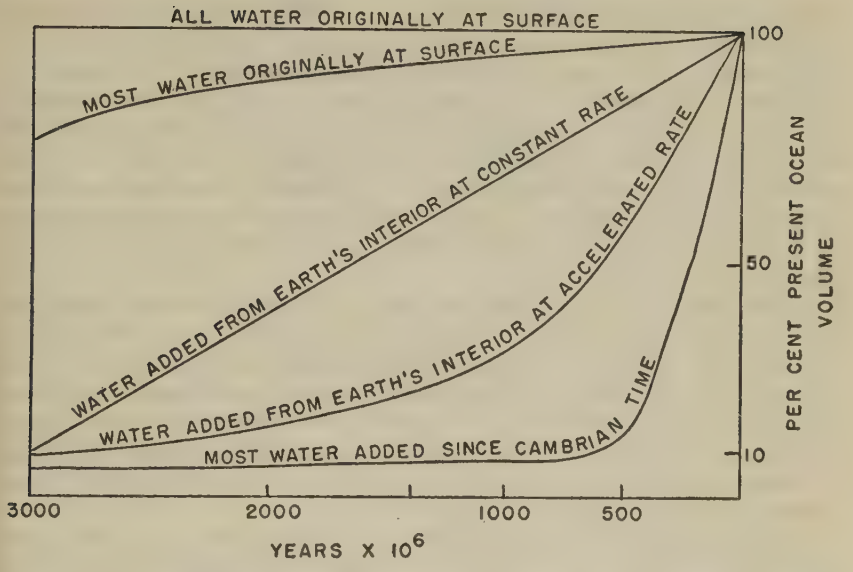

Figure 1. Possible volumes of the ocean during the geologic past, according to different hypotheses.

ever since the beginning of the geologic record the volume of ocean waters has been about the same as that which exists at present; others claim that in the earliest Archaeozoic the ocean volume was probably less than 10% of what it is today (Fig. 1).

Those (for example, see Rubey, 1951) who have argued for the slow growth of the ocean have shown that the quantity of water now flowing from volcanic springs is more than sufficient, if extrapolated back through geologic time, to account for all the water in the ocean. Furthermore, they have pointed out that in volcanic gases and in igneous rocks the ratios of the "volatiles" (water, carbon dioxide, chlorine, nitrogen, and sulphur) are similar to the ratios in the ocean, atmosphere and sedimentary rocks combined. The total amounts of these substances present on the earth's surface are roughly equivalent to the amounts contained in a layer of igneous rocks only 50 to 100 km thick, so that a relatively small loss of certain of the volatiles in igneous rocks would be sufficient to account for the amounts on the surface.

If the volatiles other than water were present to more than a minor extent at the beginning of geologic time, the earliest sedimentary rocks should be different in several respects from those laid down since. We do not believe this is the case. For example, with all

the carbon dioxide present initially, the rate of chemical weathering should have been enormous and the earliest sediments should be characterized by a higher than observed percentage of limestones. Moreover, if chlorine and sulphur existed in their present amounts and were not balanced against metal ions, the primeval ocean should have been acid but should have quickly attained through chemical weathering its present salt content. A difficulty then arises in accounting for the apparently continuous partition throughout geologic time of the sodium of igneous rocks between sedimentary rocks and the ocean.

Other evidence for a slow outflow of volatiles comes from the presence of appreciable amounts of helium 4 and argon 40 in the earth's atmosphere. These are almost entirely radiogenic, from decay of the uranium and thorium series and of potassium 40; the amounts in the atmosphere, particularly the presence of 10 to 20% of all the argon ever produced in the crust and mantle of the earth, suggest that other volatiles could also have flowed out of the earth's interior in large quantities (Birch, 1951; Kulp, 1951).

There is evidence that the present continents have grown laterally throughout geologic time (Wilson, 1954). For the most part, the oldest rocks are concentrated in the central shield areas, with younger rocks around their edges. If the growth of the continents has included the greater part of their present areas, the area occupied by the ocean has been considerably reduced and the sea floor must have become deeper relative to the continental level. Isostasy would require a thickening of the continental crust, and this profound redistribution of the earth's materials could very well have been accompanied by an outward flow of water, which would have deepened the ocean still further.

Geologists (for example, see Kuenen, 1950) who believe in the permanence of the oceans have pointed out that the water contained in the total volume of volcanic rocks produced during geologic time is far less than the volume of water in the oceans. The volatiles other than water may have slowly seeped from the interior of the earth through volcanic springs and in other ways while the water itself may have simply recirculated in the upper part of the crust (Conway, 1942). The chemical mechanism by which the water contained in igneous rocks could have been released is not clear. All the atmospheric argon may have appeared in the atmosphere in pregeologic time, because the rate of production was then much higher (Birch, 1951). However, this cannot be true of the helium 4 now present in the atmosphere, because it is known from the ratio of helium 3 to tritium (Libby, 1946) that helium escapes from the atmosphere

at such a rate that virtually all the helium initially present must have escaped.

Light may be thrown on this question of the ocean's growth by a comparison of Earth with her sister planets, Mars and Venus. The earth has sometimes been called the water planet. To an observer on Mars the most striking feature of Earth might well be the beautiful pattern of sea and land, with the continents rising like varicolored islands from the world-girdling ocean. By contrast, to us Mars is dust dry; the total amount of water in its atmosphere is sufficient to form a liquid layer less than a tenth of a millimeter thick (Kuiper, 1952: 389). A Martian observer might also note another contrast between Mars and Earth. The earth's land surface is rough with great elongate ridges which we call mountain ranges, and the red flares of volcanic activity are frequently present. By contrast, the surface of Mars appears to us to be relatively smooth (Urey, 1952: 222); there are probably no high mountains, and as we shall see, little volcanic activity.

A careful Martian observer would also conclude, by comparing the speed of rotation of the earth with its oblateness, that the earth has a core of much heavier material than the surrounding shell. Probably he would not be able to recognize what we know from seismic evidence that this core is liquid. We believe, from a similar comparison of speed of rotation and oblateness, that if Mars has a core it must be small (Brown, 1950: 641).

It is possible that these differences between Earth and Mars are related and, if so, they may help us to reconstruct the history of our own ocean. The scarcity of water on Mars has often been attributed to photochemical dissociation and the escape of hydrogen and oxygen. Because of the smaller size of Mars, its escape velocity is only about half of that from the earth, and probably some oxygen as well as hydrogen, produced by the photochemical dissociation of water, have been lost. As I shall attempt to show, however, Mars may never have had much water on its surface.

To examine this question further, let us take a more detailed look at the composition of the volatiles of Mars, Venus and Earth. Table I shows what is known about the amounts of water, carbon dioxide, oxygen, nitrogen plus argon, methane and sulphur dioxide on the surfaces of these terrestrial planets. The total mass of atmospheric gases per unit area on Mars is about 20% of that on the earth. Of this Kuiper (1952: 388) believes that perhaps 96% is nitrogen and nearly 4% argon. There is about twice as much carbon dioxide per unit area as that in the terrestrial atmosphere. Oxygen has not been detected, and, according to Dunham (1952: 299), the amount in the atmosphere is less than 0.35 g cm^{-2}. To this should be added

TABLE I. Surface Volatiles on Earth, Venus and Mars* in g cm^{-2}. Numbers Outside Parentheses Indicate Amount in Planet's Atmosphere. Numbers in Parentheses Indicate Estimated Total Amount in Atmosphere, Hydrosphere and Sediments

Substance	*Earth*	*Venus*	*Mars*
H_2O	6.25	<0.12	0.01
	(3.15×10^5)		(0.1)
CO_2	0.46	210	0.92
	(1.8×10^4)		
O_2	232		
	(1430)	<4.5	<0.35
			(50)
$N_2 + A$	770	?	190
	(850)		
CH_4	9.4×10^{-4}	<0.015	<0.008
SO_2+$SO_4^=$	10^{-9}	?	$<3 \times 10^{-9}$
	(10^3)		

* Values for Earth principally from Hutchinson (1954); for Mars and Venus from Kuiper (1952), Dunham (1952), and author's estimate of amount of oxidized iron.

the amount used in oxidizing ferrous iron to ferric iron in the surface rocks. This amount can be estimated in the following way: Assume that 10% of the planet's surface was once covered with mountains of impact origin, like those on the moon, and that the average height of these mountains was 1000 m. During the lifetime of the planet these mountains could have been leveled by wind erosion, leaving a layer of eroded material. If the mountains were conical in shape and if the eroded material was originally similar to the average terrestrial igneous rock in our continental crust, the eroded layer must be about 25 m thick, its density must be 2.7, and 8 g O_2/kg would have been required to oxidize the contained iron, or 50 g cm^{-2} over the planet's surface.

Less than 0.01 g cm^{-2} of water is present in the Martian atmosphere. The polar snow caps probably contain an amount of water equivalent to about 0.1 g cm^{-2} when spread out over the planet's surface (Kuiper, 1952; 397).

Neither methane nor sulphur dioxide have been detected on Mars, even though the methods of analysis (ultraviolet and infrared spectroscopy) are quite sensitive. The absence of sulphur dioxide in a virtually water-free atmosphere suggests the absence of vulcanism (Kuiper, 1952: 367). We will discuss the significance of the absence of methane later.

On Venus, neither water nor oxygen have been found; according to Dunham (1952: 295), the amounts present are less than 2% of those in the earth's atmosphere. The principal gas is carbon dioxide, which is present in large amounts. This fact in itself indicates the absence of water. In the presence of water, the tendency to approach the "Urey Equilibrium,"

$$Mg\ SiO_3 + CO_2\ (g) \leftrightharpoons Mg\ CO_3 + SiO_2 \quad \text{and}$$

$$Ca\ SiO_3 + CO_2\ (g) \rightleftarrows CaCO_3 + SiO_2\ ,$$

would quickly result in the production of carbonates and in the depletion of the atmospheric carbon dioxide (Urey, 1952: 148–149). As on Mars, the absence of methane is noteworthy. The amount of nitrogen plus argon is not known, but it may be quite large. These gases cannot be detected spectroscopically and their presence on Mars is inferred from other observations.

If Mars originally had an appreciable quantity of water and nitrogen, but no oxygen, on its surface and in its atmosphere, then some solar ultraviolet radiation should have penetrated to the surface and some water would have been dissociated photochemically into hydrogen and oxygen. The hydrogen would escape rapidly from the planet, the oxygen much more slowly, at a rate determined by the temperature in the Martian exosphere. Some oxygen would also be withdrawn from the atmosphere by oxidation of iron in surface rocks. The oxygen remaining in the atmosphere would absorb the ultraviolet radiation before it reached the planet's surface. Further dissociation of water would then be confined to the outer region of the atmosphere. Just as on Earth, there should have been at least one and perhaps two atmospheric layers of very low temperature, between 150° and 200° K. Harteck and Jensen (1948: 591) and Kuiper (1952: 310–314) have pointed out that on Earth these low temperature layers and the overlying layers of increasing temperature act as a "cold trap" through which water can pass only very slowly. Kuiper has estimated that the rate of flow of water through the stratospheric cold trap on Earth is 1.4×10^{-14} g cm^{-2} sec^{-1} or 1.5 kg cm^{-2} in 3.5×10^9 years. He suggests that, because of the lower temperature in the corresponding atmospheric layer on Mars, the rate of outward flow of water is perhaps 100 times slower. In any case, the oxygen content of the Martian atmosphere should have increased rapidly to such an amount that a steady state equilibrium would exist between the rate of transfer of water to the upper atmosphere and the rate of escape of oxygen from the exosphere. This equilibrium may well exist at the present time. Dunham's (1952)

upper limit for oxygen in the Martian atmosphere, 0.35 g cm^{-2} (Table I), is about 50 times the amount of oxygen required in the earth's atmosphere to absorb solar radiation with wave lengths between 1800 and 2000 angstroms (Goldberg, 1954: 748). Radiation of these wave lengths is responsible for photochemical dissociation of water in the outer layers of Earth's atmosphere (Bates, 1954: 591–592). Thus, photochemical dissociation of water may now occur only some tens of kilometers above the solid surface of Mars (Fig. 2).

Spitzer (1952: 239–244) has recently formulated the relationship between escape velocities for atmospheric gases and temperature in the exospheres of Earth and Mars. His results are confirmed as to order of magnitude by Libby's (1946: 671) essentially experimental determination of the rate of escape of helium 3 from the earth's

TABLE II. Times Required for Nitrogen and Oxygen to Decrease by 1/e Through Escape from Martian Atmosphere, at Different Exosphere Temperatures

Temperature in °K at base of Martian exosphere	*Time of escape in years*		
	Nitrogen		*Oxygen*
	$N/N_2 = 0.01$	$N/N_2 = 0.001$	
1200	5.0×10^9	5.0×10^{10}	5.9×10^8
1300	1.7×10^9	1.7×10^{10}	1.5×10^8
1400	6.1×10^8	6.1×10^9	4.7×10^7
1500	2.5×10^8	2.5×10^9	1.7×10^7
1600	1.2×10^8	1.2×10^9	6.4×10^6
1700	5.9×10^7	5.9×10^8	2.9×10^6
1800	3.4×10^7	3.4×10^8	1.6×10^6

atmosphere, based on the ratio of helium 3 to tritium. Table II, computed from Spitzer's equations, shows the times required at different exosphere temperatures for nitrogen and oxygen in the Martian atmosphere to decrease by 1/e. The escape time for nitrogen depends on the degree of dissociation of molecular nitrogen into the atomic form at the base of the exosphere. The percentage dissociation has been estimated as being between 0.01 and 0.001 by both Bates (1954: 589) and Nicolet (1954: 687). If we assume that the total amount of nitrogen ever present in the atmosphere of Mars is equal to the nitrogen now in the earth's atmosphere, the Martian exosphere temperature lies between 1300° and 1700° K (Table III). The actual value depends: (1) on whether all the nitrogen was originally present 3.5×10^9 years ago or has come from the interior during the planet's lifetime; and (2) on the ratio of molecular to atomic nitrogen in the exosphere. The range of escape times for oxygen over this tempera-

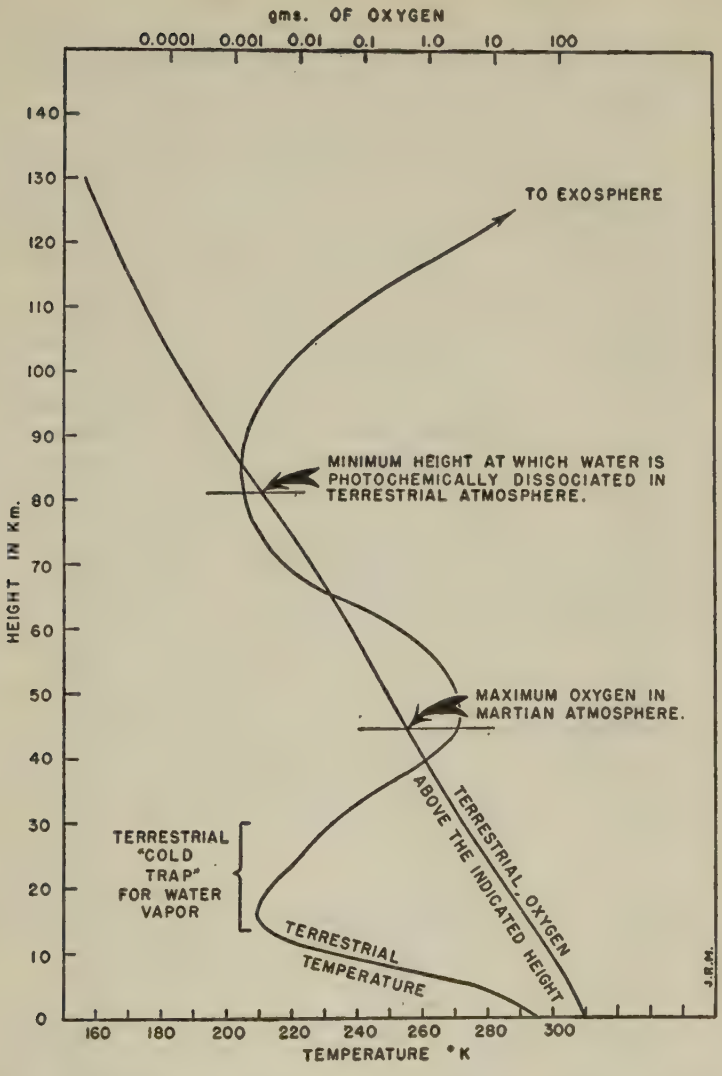

Figure 2. Temperature and oxygen content of the terrestrial atmosphere, showing the minimum height for photochemical dissociation of water, the "cold trap" for upward transport of water and the height above which the oxygen of the terrestrial atmosphere is equal to the maximum oxygen content of the Martian atmosphere.

TABLE III. Ratios of Total Martian and Terrestrial Nitrogen and Water, per Unit Area, During 3.5×10^9 Years, for Different Martian Exosphere Temperatures

$$\frac{\text{Earth N}}{\text{Mars N}} = \frac{850}{\text{Mars N}} \qquad \frac{\text{Earth water}}{\text{Mars water}} = \frac{3.15 \times 10^5}{\text{Mars water}}$$

Temperature at base of Martian exosphere, °K	*All Mars nitrogen originally present on surface*		*All Mars nitrogen derived from planet's interior at constant rate*		
	$N/N_2 = 0.01$	$N/N_2 = 0.001$	$N/N_2 = 0.01$	$N/N_2 = 0.001$	
1200	2.2	4.2	3.2	4.4	5.3×10^3
1300	1.5*	3.6	1.9*	4.0	4.8×10^3
1400	.13	2.6	.79	3.4	3.7×10^3
1500	10^{-5}	1.1*	.32	2.4	2.3×10^3
1600	10^{-12}	.27	.15	1.5*	1.2×10^3
1700	10^{-25}	.14	.076	.76	5.9×10^2
1800	10^{-44}	2×10^{-4}	.043	.43	3.4×10^2

* Indicates maximum probable temperature at base of Martian exosphere (temperature at which total Mars nitrogen, in g cm^{-2} $\approx$ total terrestrial nitrogen, in g cm^{-2}).

ture range is 1.5×10^8 to 2.9×10^6 years, and the amount of oxygen which has escaped, plus the amount used in oxidizing iron, lies between 60 and 470 g/cm^{-2}. As shown in Table III, the total amount of water on Mars throughout its history was less than one six-hundredth of the water now present on the earth, provided the equilibrium oxygen content of the Martian atmosphere is 0.35 g cm^{-2}.

From the above considerations it seems likely that Mars never had more than a very shallow ocean. It is not difficult to believe that this is so, because large quantities of water have never seeped out of the planet's interior. This in turn may be related to the apparent absence of mountains and volcanoes. Mars may never have had the turmoil within its interior that has characterized the earth throughout its lifetime. Conversely, it does not appear illogical that the great volume of the terrestrial ocean is a direct result of the continuing turmoil within the earth.

It is possible, as suggested by Urey (1952: 149–157), that the earth's primitive atmosphere contained large amounts of water, ammonia and methane. In the absence of free oxygen, a cold trap might not exist, and moreover ultraviolet radiation could have penetrated to near the ground. Then the initial atmospheric methane would have become oxidized to carbon dioxide within a few hundred million years as a result of the photochemical dissociation of methane and water and the escape of hydrogen. (At the same time the ammonia would have become oxidized to free nitrogen.) If this initial methane corresponded in amount to the total CO_2 present in oceans, atmosphere and sediments, intense chemical weathering should have occurred and vast amounts of sediments, including carbonates particularly, should have been deposited in the very early stages of the earth's history. This does not seem to be consistent with geologic evidence.

Methane may not have been present originally in appreciable quantity but may have come slowly to the surface, but we must then explain why present volcanic gases contain CO_2 and CO and not methane. If the methane did not come in as rapidly as hydrogen escaped, conditions should soon have become oxidizing and a cold trap should have arisen. The rate of flow of water through the cold trap required to produce all the CO_2 by oxidation of methane is 10 times greater than that computed by Kuiper.

It is possible that the methane appeared rapidly enough in the early stages of the earth's history so that conditions remained reducing for a long period and under those conditions no free oxygen and no cold trap existed. During later geologic time the rate of outflow decreased to the point where free oxygen could appear. This would imply

that nearly all carbon dioxide now present in the atmosphere, ocean, and sedimentary rocks was produced during early geologic time. Of course, we cannot invoke the hypothesis of photosynthetic production of oxygen and burial of reduced carbon in the form of organic matter in marine sediments to account for production of carbon dioxide and free oxygen from methane and water.

In attempting to resolve this question we may look again with profit to our sister planets Mars and Venus. Urey (1952: 148–150) has suggested that the high concentration of CO_2 and the absence of water in the atmosphere of Venus are due to the photochemical dissociation of methane and water and the resulting oxidation of carbon. But both water and methane are now virtually absent on Venus. Urey's hypothesis would explain this observation if: (1) the amounts of water and methane originally present were just sufficient to balance each other, a coincidence that appears quite unlikely when we remember that both water and methane are also scarce on Mars; or (2) there was an excess of methane, which decomposed to carbon and perhaps refractory carbon compounds. Urey (personal communication) has pointed out that the light yellow color of the Venus cloud layer would appear to rule out the presence of finely divided carbon, which should give a dark "smoky" color.

As several writers have emphasized, the existence of reducing conditions during early stages of the earth's history may have been an essential prerequisite for the origin of life on our planet. The complex organic molecules that are the building blocks of living creatures probably could not have evolved in an oxidizing environment. Thode and his coworkers (Szabo, *et al.*, 1950), from studies of sulphur isotope ratios in sulphides and sulphates of different geologic ages, have suggested that free oxygen did not exist in large amounts until perhaps 800 million years ago. If this were correct, then we might have a satisfactory explanation for the absence of animal fossils in pre-Cambrian rocks and an explanation also for the sudden appearance of fossil remains in many diverse, highly developed animal forms in the Cambrian.

With the growth of biochemical knowledge, the extreme chemical complexity of living things is becoming ever more apparent (see, for example, Wald, 1954). Nearly all this complexity is possessed by even the simplest organisms. It may be that by far the most difficult and time-consuming part of the evolutionary process was the development of the chemistry of the single cell, the building block of all living creatures. This development, including the processes of fermentation, then photosynthesis, and finally respiration could have been carried out entirely by plantlike organisms, which, unlike animals, require no external source of oxygen for their metabolic processes.

Given the essential chemistry for the organization of living cells, the evolution of animals represents little more than mutations required for the development of a complex morphology, and conceivably they might have happened within a few hundred million years. If we remember that precipitation of calcium carbonate, though easy for plants, which raise the pH by removing carbon dioxide, is a most difficult process for animals, whose principal metabolic products are acid, then it seems likely that the first animals had no hard parts. Considerable diversity of form might have arisen before animals learned how to manufacture shells or skeletons. But once this process was learned, its advantages would be so overwhelming that few animals could survive for long without hard parts.

Thus the sudden appearance of animal fossils in the Cambrian strongly suggests that appreciable amounts of free oxygen did not exist more than a few hundred million years before that time. How, then, can we reconcile the need for reducing conditions in relatively late geologic time with the difficulties previously discussed concerning methane? I believe the answer has been given by Rubey (1951). He has reminded us that volcanic gases contain both CO_2 and CO and has suggested that the organic carbon in the sedimentary rocks can be balanced against the oxygen in the atmosphere, in sulphates and in ferric oxide, provided that approximately a third of the total carbon came to the earth's surface as CO and two-thirds as CO_2. During the early stages of the earth's history, the existence of CO in the atmosphere and oceans would have assured reducing conditions, at least in some parts of the oceans and at times elsewhere. Free oxygen would appear in significant quantities only after the process of photosynthesis had been developed and after sufficient reduced carbon as organic matter had been buried in marine sedimentary rocks.

The reader may well have noted that comparisons of the three terrestrial planets given in preceding pages raise several unanswered questions. For example, why was there more nitrogen than water on Mars and less on the earth? Why is there virtually no water or carbon monoxide on Venus?

It is interesting in this connection to note that the scarcity of oxygen on Mars may be partly an oceanographic phenomenon in reverse. Mars has no oceans, and therefore there is no possibility for the formation of marine sediments with their contained organic remains. Thus, if primitive plants exist on Mars, as seems probable, the oxygen produced by their photosynthesis is used up again by respiration so that none accumulates.

According to Rubey (1951), the geologic evidence indicates that the concentration of CO_2 in the atmosphere and ocean has always been

approximately the same as that now existing. From this he concludes that the rate of outflow of volatiles from the earth's interior has been relatively constant. But in the presence of water, the Urey equilibrium would insure a relatively low concentration of atmospheric and oceanic carbon dioxide at all times regardless of the rate of outflow from the interior. At times of more rapid outflow the rates of chemical weathering and of deposition of limestones and other sediments would be increased; at times of slow outflow the rates of weathering and deposition would be slow, other things being equal.

There is some evidence that a considerable portion, perhaps as much as a fourth of all the water in the oceans, has appeared since the late Mesozoic, that is, in the last thirtieth or fortieth of geologic time. Throughout a large area within the "andesite line" in the Pacific Ocean, the presence of coral atolls and flat-topped sea mounts indicates subsidence of the sea floor relative to sea level. This subsidence may have been regional in character, and if so, the approximate constancy of sea level relative to the continental surfaces would require an increase in the total volume of sea water. There is good evidence that, if the subsidence did occur, it took place during the last 100 million years. The fossils found in the drill holes on Bikini and Eniwetok by Ladd and his coworkers (Emery, *et al.*, 1954) are at all depths those of shallow-water, mostly lagoon-dwelling, organisms. At 2500 feet on Bikini, these were early Miocene or late Oligocene in age; at 4500 feet on Eniwetok the fossils were Eocene. Remains of reef corals, echinoids and shallow-water mollusks, dredged from the flat-topped summits of the submerged mid-Pacific mountains between Hawaii and Wake Islands, have been identified by Hamilton (1953) as Middle Cretaceous in age. These summits now lie at depths of 5000 to 6000 feet.

Evidence for a marked increase in the rate of outflow of volatiles may be found also in the character and rate of deposition of deep sea sediments. At the present time, nearly half the deep sea floor is covered with sediments high in calcium carbonate, such as globigerina ooze. These are being deposited at rates of the order of 0.1 to 1 g $Ca/m^2/1000$ years (Kuenen, 1950). If this rate of deposition has prevailed since the late Mesozoic, that is, since the rise of the pelagic foraminifera, then about a fourth of all limestones now on the earth's surface has been laid down during the last 100 million years. Prior to that time, the deep sea sediments must have been low in calcium. Otherwise, it would be impossible to arrive at a geochemical balance, because both deep-sea and continental sediments would be higher in calcium than the igneous rocks from which they were derived. Because of this change, Kuenen (1950) has suggested that, with the

rise of pelagic foraminifera, the rate of deposition of calcium carbonate on the deep sea floor has been increased at the expense of the shallow water areas. He supposes that a gradual shift in the location of the sedimentary calcium on the earth is occurring; limestones on the continental platforms are being eroded, and calcium carbonate is accumulating on the deep sea floor, where it will be lost from the calcium cycle. But an alternative explanation is possible: that during late geologic time there has been a great increase in the rate of outflow of carbon dioxide from the earth's interior, with a corresponding increase in the rate of chemical erosion of rocks other than carbonates and in the rate of sediment deposition. The increase in carbon dioxide should have been accompanied also by a corresponding increase in water and other volatiles. The increased supply of calcium from erosion may have stimulated the development of the pelagic foraminifera.

That great events took place in the sea floor in the not distant geologic past is strongly suggested by the extreme thinness of deep sea sediments, as found in both the Atlantic and Pacific by seismic refraction methods. Over a large area in the Pacific, Raitt (Capricorn, 1953) has found that these sediments (at least in unconsolidated form) are only 100 or 200 m thick. A far greater thickness of deep sea sediments is called for by geochemical considerations. The earlier deep sea sediments may have been buried or consolidated in extensive lava flows or other volcanic extrusions that blanketed the sea floor, perhaps during the Mesozoic. Such vast-scale vulcanism should have been accompanied and succeeded by an outpouring of volatiles that may have persisted to the present time.

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METEOROLOGY IN OCEANOGRAPHY

ON THE EXCHANGE OF MOMENTUM BETWEEN THE ATMOSPHERE, THE OCEANS, AND THE SOLID EARTH

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There is general agreement now among oceanographers that the large ocean currents are essentially maintained by the stress exerted by planetary wind systems on ocean surfaces. Therefore it is of primary importance for oceanography to know the distribution of the wind stress on oceans and its variation in time.

The wind stress τ_0 has generally been expressed in terms of a resistance coefficient, γ^2, defined by

$$\tau_0 = \gamma^2 \rho_a V_0^2. \tag{1}$$

Here V_0 denotes the wind speed at a level near the surface (anemometer level) and ρ_a the density of the surface air. The resistance coefficient γ^2 is not a real constant, since it depends upon the roughness of the sea surface. Thus, in the case of a water surface, roughness depends partly on the waves formed by the wind. Different investigations have produced different values of the resistance coefficient, and an interesting summary of the present disagreement concerning the value of the resistance coefficient is found in a recent paper by Montgomery (1952), who summarizes the situation as follows: "The present evidence is so conflicting that at no wind speed is the resistance coefficient known confidently within its half value."

For oceanography it is probably less important than for other fields of study to have more or less exact measurements of the wind stress under given conditions; it is probably sufficient for oceanographers to know reasonably well the mean wind stress on large ocean areas and over extended periods of time. In the following paragraphs some of the methods which could be used for a computation of the global distribution of wind stress will be discussed.

The global wind system is characterized by the prevailing easterlies, roughly between Lat. 30° N and 30° S, and by the prevailing westerlies polewards from these latitudes. This large-scale planetary wind system results in a characteristic exchange of angular momentum between the earth, including the oceans and the atmosphere. In the belt of easterlies, the earth, due to the interfacial stress, exerts

a torque on the atmosphere. The rotation of the atmosphere in the tropics is therefore speeded up and its westerly angular momentum increases. In the belts of westerlies, the interfacial stress acts in an opposite direction and the rotation of the atmosphere in those regions has a tendency to be reduced. The principle of conservation of angular momentum means that the total angular momentum of the solid earth, the hydrosphere, and the atmosphere combined remains constant. For periods of time ranging from a few days to seasons there could be observable changes in the angular momentum of the solid earth, the hydrosphere and the atmosphere taken separately; seasonal changes in this respect have been observed and have been found to be in agreement with the dynamic theories. However, those changes are of minor importance for our problem, and, on the whole, we can assume therefore that the angular momentum of the atmosphere, the oceans, and the solid earth taken separately remains constant for extended periods of time.

This principle means that there must be a continuous flux of angular momentum in the earth's atmosphere from the "source region" in the tropical and subtropical easterlies to the "sink regions" in the extratropical westerlies. The maximum meridional flux of angular momentum then occurs in the latitudes that separate the zone of easterlies from the belts of westerlies. Hence the total interfacial stress in the trade-wind belt multiplied by the distance from the earth's axis must equal the interfacial stress in the zones of westerlies multiplied by the distance from the earth's axis in these regions, but with opposite sign.

This principle can be used for a computation of mean surface stress in the principal wind belts. If it is possible to determine the poleward flux of angular momentum in the atmosphere across every latitude, these fluxes would also determine the mean surface stress in every singular latitudinal belt.

In recent years extensive investigations have been carried out in this field. Without going into detail I especially want to refer to the important work done on this subject at Massachusetts Institute of Technology by Starr and White (1952) and others, at the University of California at Los Angeles by Bjerknes (1951) and Mintz (1951), and in Australia by Priestley (1951). Due essentially to the results of this work, we know reasonably well the mean flux of angular momentum in the meridional direction as a function of latitude, and consequently we are also in a position to compute the mean surface stresses in selected latitudinal belts.

As an example, the fluxes of angular momentum in January and July across Lat. 30° N are presented in Fig. 1. The amount of

angular momentum carried through this latitude in January averages about 50×10^{25} g cm^{2}sec^{-2} and in July about 15×10^{25} g cm^{2}sec^{-2}. According to the principle presented previously, these values also represent the angular momentum which is brought up into the atmosphere between Lat. 30° N and the equator and which is carried down to the earth north of 30° N. The corresponding angular momentum flux through Lat. 30° S is not so well known due to lack of meteorological data from the southern hemisphere. However, from available data, this flux could be estimated to be about 35×10^{25} units in January and about 55×10^{25} units in July. Therefore, the total torque in the trade-wind belt due to surface wind stress, in the most general meaning of the word, amounts to about 85×10^{25} g cm^{2}sec^{-2} in January and about 70×10^{25} g cm^{2}sec^{-2} in July. From

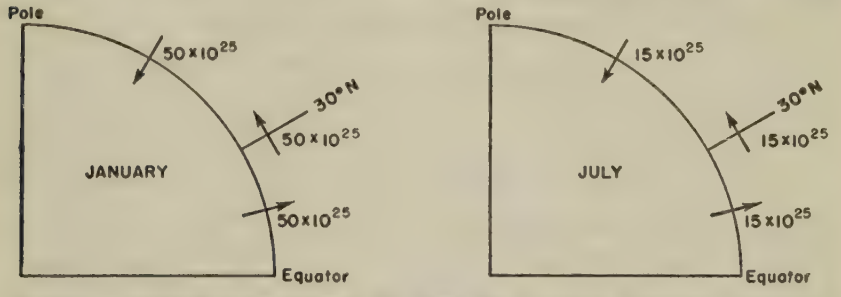

Figure 1. Schematic figure showing the mean flux of angular momentum across Lat. 30° N in July and January (essentially according to Mintz); unit g cm^{2}sec^{-2}.

these values the mean surface wind stress in the whole trade-wind zone can be evaluated.

A part of this exchange of angular momentum represents a direct exchange between the atmosphere and the solid earth and another part represents an exchange between the atmosphere and the oceans. This latter part could be computed if the ratio of oceans and continents and, in addition, the mean surface stress in these different areas were known. Before we discuss this question, a few words should be said concerning the mechanism of the meridional flux of angular momentum in the atmosphere.

According to the investigations referred to previously, the meridional flux of angular momentum in the atmosphere across Lat. 30° N and 30° S is essentially performed by large-scale atmospheric disturbances of the type of long waves, cyclones and anticyclones, and so on. Consequently the flux can be considered as a result of different kinds of large-scale eddy stresses, "Reynolds stresses," between the tropical and extratropical atmosphere. These large-scale eddies are especially

effective in the middle and upper parts of the troposphere and in the tropopause region, as has been shown by Mintz (1951) and by Starr and White (1952), As a result of this, the momentum flux across the latitudes in question occurs essentially in the layers between 700 and 100 mb, or between 10,000 and 55,000 feet. It is of considerable interest that computations carried out for atmospheric layers so far removed from the sea surface can be used to determine wind stress on ocean surfaces.

The interfacial stress, however, can also be determined from wind observations at lower levels. As has already been mentioned, the conventional method is to use eq. (1). Another method, probably first used by Richardson (1920) and later by Rossby and Montgomery (1935) and several others, is based on the departure from geostrophic wind in the frictional layer.

The equation of motion can be written in the form

$$\rho \frac{du}{dt} = 2\Omega \sin \phi \rho v - \frac{\partial p}{\partial x} + \frac{\partial \tau_{xz}}{\partial z}$$

$$\rho \frac{dv}{dt} = -2\Omega \sin \phi \rho u - \frac{\partial p}{\partial y} + \frac{\partial \tau_{yz}}{\partial z} . \qquad (2)$$

Here u, v denote the zonal and meridional components of the air motion, ρ the density of air at an arbitrary level, Ω the angular velocity of the earth's rotation, ϕ the latitude, and p the pressure; τ_{xz}, τ_{yz} denote the components of shearing stress in the direction of the axes. In these equations some small terms have been neglected. By integrating these equations, the difference in shearing stresses between the earth's surface and an arbitrary level H can be determined:

$$\tau_{xH} - \tau_{x0} = -2\Omega \sin \phi \int_0^H \rho (v - v_g)\, dz + \int_0^H \rho \frac{du}{dt} dz$$

$$\tau_{yH} - \tau_{y0} = 2\Omega \sin \phi \int_0^H \rho (u - u_g)\, dz + \int_0^H \rho \frac{dv}{dt} dz . \qquad (3)$$

Here τ_{xH}, τ_{x0} and τ_{yH}, τ_{y0} denote the components of the stresses at the level H and the surface; u_g, v_g denote the components of the geostrophic wind.

If we apply these equations to the mean motion of a total latitudinal ring in the lower atmosphere, then the terms containing the acceleration components become relatively small and can be neglected, as a

first approximation. In this case also the mean geostrophic meridional component, $\bar{v}_g$, vanishes. Thus we get

$$\bar{\tau}_{x0} = \bar{\tau}_{xH} + 2\Omega \sin \phi \int_0^H \rho \, \bar{v} \, dz$$

$$\bar{\tau}_{y0} = \bar{\tau}_{yH} - 2\Omega \sin \phi \int_0^H \rho \, (\bar{u} - \bar{u}_g) \, dz \, . \qquad (4)$$

The bars denote mean values for a total ring of latitude.

The components of shearing stress in the atmosphere can be written

$$\tau_{xH} = \left(\mu \frac{\partial u}{\partial z}\right)_H , \quad \tau_{yH} = \left(\mu \frac{\partial v}{\partial z}\right)_H , \qquad (5)$$

where μ denotes the coefficient of eddy viscosity. In the atmosphere, with its poleward-directed temperature gradient, the geostrophic west component increases with height. In the trade-wind belt the vertical shear of the real zonal component is therefore negative in the vicinity of the earth's surface and passes a level of no shear (level of maximum easterlies). The level of no vertical zonal shear, H_1, is then also a level of no vertical eddy flux of momentum. In the zones of surface westerlies, no corresponding level of zero stress exists.

If the integration in (4) is extended from the surface to the level H_1, the zonal component of the surface stress can be determined by the simple formula

$$\bar{\tau}_{x0} = 2\Omega \sin \phi \int_0^{H_1} \rho \, \bar{v} \, dz = 2\Omega \sin \phi \, [m_\phi]_0^{H_1} \quad , \qquad (6)$$

where $[m_\phi]_0^{H_1}$ denotes the mean meridional mass flow through the unit length of the parallel ϕ in the layer between the surface and the level of maximum east wind.

An attempt was made to compute the mean zonal and meridional wind components at different levels in the vicinity of the latitude of strongest easterlies (around Lat. 12° N in winter), and the stations used for that purpose are marked in Fig. 2. The complete results will not be presented here. However, a similar computation was made for the time period January–February 1952 for only eight of those stations which could be considered as real ocean stations. The result is presented graphically in Fig. 3, with a linear pressure scale as ordinate. The mean values of the u-components are marked by

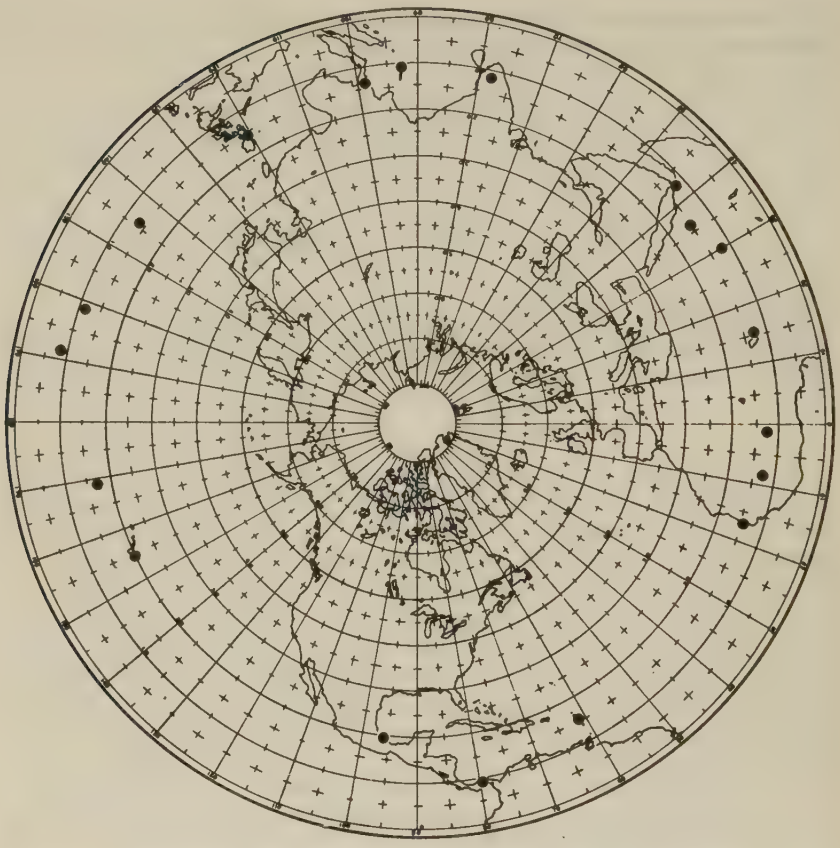

Figure 2. Circumpolar wind stations used to compute the mean distribution of the zonal and meridional wind components with height in the vicinity of Lat. 13° N.

crosses at the levels of wind observations, whereas the mean values of the v-components are marked by rings. The mean level of maximum east wind is 915 mb, and the mean level of maximum north wind is 957.5 mb. The components of surface stresses are proportional to the areas A and B, shaded in the diagram. For mean values of τ_{x0} and τ_{y0} we get the values -1.0 and -0.6 dynes/cm^2 respectively.

This example shows how the mean zonal and meridional components of surface stress could be determined for different latitudes if sufficiently good wind data were available. Unfortunately the network of upper wind stations is not dense enough over the ocean areas to permit a detailed computation of surface stresses on a global scale.

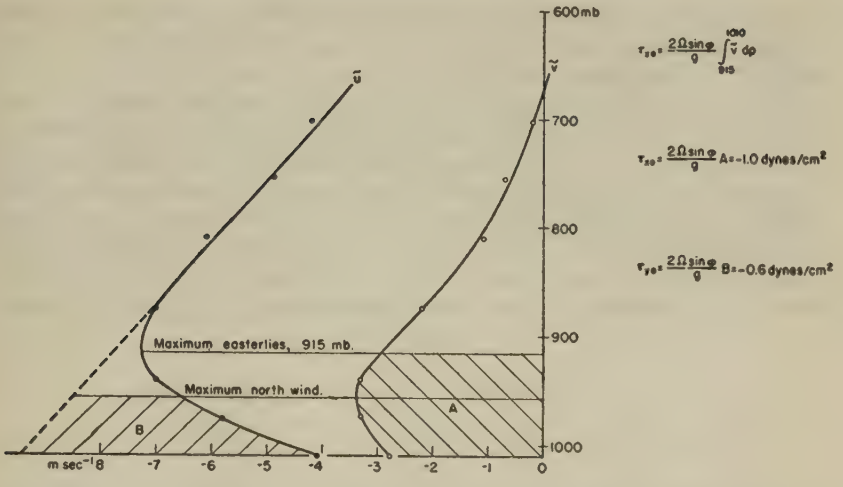

Figure 3. Mean distribution of the zonal and meridional wind components, with height, over eight selected ocean stations (linear pressure scale). The shaded areas A and B are proportional to the total ageostrophic meridional and zonal mass flow used for the computation of components of surface stress given. The dashed curve indicates the mean geostrophic zonal wind.

The above values are valid for January and February and for the mean Lat. 13° N, where the acceleration terms in (3) approximately vanish. They seem to be in fair agreement with the corresponding values of the stress computed by Priestley (1951) from surface wind data by using equation (1). Our values, however, are somewhat higher.

Since, in the zones of westerlies, no level of vanishing τ_{xH} exists in the lower atmosphere, the surface stress here cannot be computed directly from the departure from geostrophic wind in the frictional layer. This important difference between the zones of easterlies and the zones of westerlies was first pointed out by Sheppard, *et al.* (1952) and by Sheppard and Omar (1952). As can be seen from (4), the mean ageostrophic mass flow is proportional to the difference between surface stress and stress in an arbitrary level, not to the surface stress itself. This seems to be the most probable explanation of the fact that computations of surface stress from the departure of the wind from geostrophic flow generally have resulted in considerably smaller values than those computed in other ways (Montgomery, 1952).

Fig. 4 shows schematically the difference of momentum flux in cases of surface easterlies and westerlies. For an east wind, westerly momentum is transported upwards below the level of maximum easterlies and downwards above that level, whereas in the case of a

west wind the flux of westerly momentum always is directed downwards. The departure from geostrophic wind depends on the convergence in this momentum flux.

Comparison of the three different methods used to compute mean zonal stress in the trade-wind belt shows fair agreement in the results. All three methods have some disadvantages. The method based on surface wind suffers from the disadvantage of using a drag coefficient, not well defined, which can vary with both wind velocity and characteristic roughness of the surface. The method based on divergence of the meridional flux of angular momentum depends upon the existence of sufficiently good wind observations up to high altitudes.

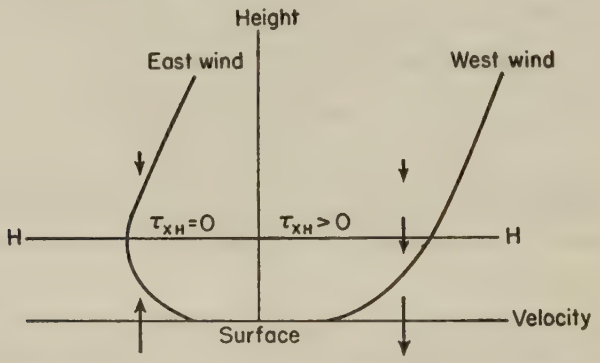

Figure 4. Schematic figure showing the difference in vertical eddy flux of momentum in the zones of easterlies and of westerlies. The curves represent the characteristic profiles of the zonal wind components with height; the arrows indicate the direction and amount of eddy flux of westerly momentum.

Also the third method, based on departure from geostrophic flow in the lower atmosphere, depends entirely upon the existence of a good network of wind stations evenly distributed over the oceans.

Priestley has determined from surface wind data that the mean zonal stress in January for the zone Equator $-30°$ N is about -0.5 dynes/cm^2. From values of the total flux of angular momentum in the atmosphere through Lat. 30° N, a mean zonal stress of about -0.60 dynes/cm^2 results. As shown previously, the same value was obtained from the mean meridional mass flow in the frictional layer. It is probable that the somewhat higher values computed from meridional flux of angular momentum and meridional mass flow are influenced by the stronger friction over continents than over oceans due partly to zonal pressure forces acting upon mountains.

From these values of mean zonal surface stress in the northern trade-wind belt, the corresponding mean stress in the zone of surface

westerlies can be computed by using the principle that the total surface torque in both zones must be in balance. In that case there is no net change of angular momentum of the atmosphere. However, for a complete budget of angular momentum we also need a budget for the exchange between the solid earth and the oceans.

From the previous discussion it follows that, over longer time periods, the total angular momentum of every latitudinal strip of the oceans must remain constant. This rule can be expressed approximately by the equation

$$\int_0^L \tau_{z0}\, dx = -2\Omega \sin\phi \int_0^L \int_0^D \rho_w v_w\, dz\, dx + \int_0^L \int_0^D \frac{\partial p}{\partial x}\, dz\, dx - \int_0^L \int_0^D \frac{\partial \tau_{xy}}{\partial y}\, dz\, dx \quad , \quad (7)$$

where the first integral denotes the total mass transport in meridional direction in the water layer influenced by the wind, the second integral is the total pressure acting in the zonal direction, and the third integral determines the stress on the lateral boundaries of the strip. The indices w denote that we are now dealing with movement in the water; D is the depth of the water layer considered, and L is the zonal width of the ocean. At depth D the frictional stress vanishes.

For an entire zonal strip of the ocean, the net meridional mass transport can be considered zero. If we neglect the meridional stresses, about which we know little, eq. (7) can be written

$$\int_0^L \tau_{z0}\, dx = \int_0^L \int_0^D \frac{\partial p}{\partial x}\, dz\, dx \quad . \quad (8)$$

Eq. (8), which determines the zonal pressure gradient in the water as a function of the zonal wind stress in the absence of any bottom and lateral stress, has been used to determine the wind stress from the slope of the isobaric surfaces in the direction of the wind.

In a schematic rectangular ocean extending over the hemispheric belt of easterlies and the belts of westerlies, the slope of the ocean surface should have the characteristics shown in Fig. 5, if D is constant. Applied to real oceans, however, the influence of the currents induced by wind stress should also be considered. This problem has been studied by Sverdrup (1947) and in greater detail by Munk (1950).

In order to get an idea of the slope of the ocean surface that is

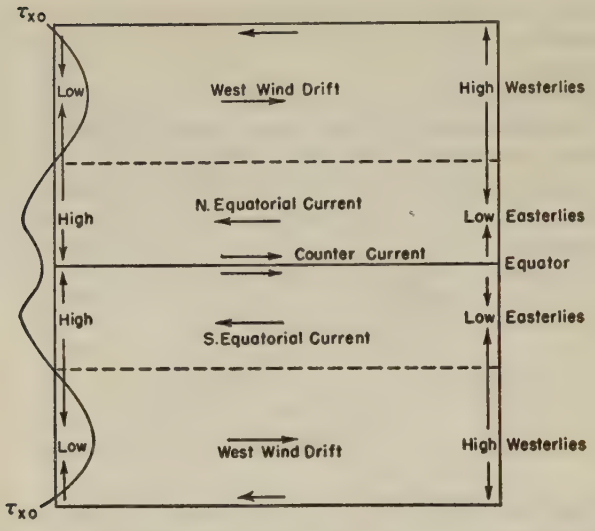

Figure 5. Schematic picture showing the zonal wind stress, the resulting height of the ocean surface, and the corresponding currents in a rectangular ocean. The influence of the currents upon the topography of the ocean surface is not considered in this figure.

necessary to compensate for the momentum transferred to the ocean from the atmosphere, we make the assumption that the ocean covers the whole northern trade-wind belt except for a continental barrier of no zonal extension along a given meridian. If the pressure gradient is constant in the whole layer with depth D, assumed to be 200 m, and if the mean wind stress is 0.5 dynes/cm^2, there would be a pressure difference of 10 cb between the eastern and western side of the barrier. This pressure difference corresponds approximately to a difference in height of 100 cm.

Considering the width of the three oceans in the northern trade-wind belt, in January the surface of the Atlantic Ocean would have a total slope of about 17 cm, the Pacific Ocean would have a slope of 47 cm, and the Indian Ocean one of 15 cm. In July the slope of the Atlantic and Pacific oceans would be considerably smaller, whereas the slope of the Indian Ocean north of the equator would have an opposite sign due to the southwest Monsoon. The real topography of the ocean surface, however, is much more complicated. As was mentioned before, the topography is determined not only by the wind stress but by the depth D and the circulation in the upper layer caused by the planetary wind system.

In computing these approximate values of the zonal component of

slope of ocean surfaces, the exchange of momentum due to lateral stresses has been neglected. Lateral stresses, however, only exchange momentum between different parts of the oceans, and our purpose was to study only the mechanism of the exchange of momentum between oceans and continents. For a complete theory it would be necessary to evaluate the net transfer of angular momentum between the atmosphere and all oceans and, considering the possible frictional transfer at the continental boundaries and at the bottom of oceans, determine the total pressure force. This, however, is not possible at the present time.

In parts of the Antarctic Circumpolar Current no continental barriers exist. Consequently here no zonal pressure force can act around the globe. It is necessary, therefore, to consider the possible existence of a relatively strong zonal bottom current in this special part of the oceans, as Munk and Palmén have pointed out (1951).

Formula (8) has also been used for computation of the mean wind stress on a water surface. When dealing with enclosed basins of larger seas or with lakes where the wind under favored conditions can be considered approximately constant, this formula probably gives the best values that can be achieved. The integrated form,

$$\lambda \tau_0 = \rho_w g DS, \tag{9}$$

was first used by Ekman (1905) for the Baltic Sea and later by several others (Hela, 1948; Palmén and Laurila, 1938). In (9), g is the acceleration of gravity, S denotes the slope of the free surface, and λ is a constant which depends upon the depth of the water and which approaches 1 if the depth is so large that bottom friction can be neglected.

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THE GENERATION OF OCEANIC CURRENTS BY WIND[1]

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1. *Introduction.* In 1937 and 1938, C.-G. Rossby published results of investigations into the manner in which velocity and pressure adjust themselves in ocean currents. He showed that an initially unbalanced rectilinear current would execute transverse inertial oscillations in the process of creating the pressure field that is necessary to balance the Coriolis forces associated with its momentum. The energy which flows into these oscillations depends on the rapidity with which the currents are built up, i. e., on the extent of the unbalance. Since large-scale atmospheric wind systems have characteristic periods large compared to the half pendulum day whereas inertio-gravitational oscillations have periods less than a half pendulum day, one would expect the bulk of energy to flow directly into the balanced current. It appeared desirable, therefore, to develop a formalism that is specifically applicable to the study of such large-scale balanced motions. In 1947 and 1948 the writer developed just such a formalism for treating large-scale atmospheric disturbances. It is the purpose of the present article to show how an analogous treatment may be applied to large-scale oceanic currents. Specifically, it is proposed to again consider Rossby's problem, but this time with the explicit assumption that wind stresses impart momentum slowly enough for transverse horizontal and vertical motions to create the Coriolian mass adjustment almost instantaneously.[2]

[1] This paper is based on a lecture entitled "Waves and Currents in Atmosphere and Ocean" delivered at the Oceanographic Convocation at Woods Hole on June 24, 1954. Part of the work was done at the Woods Hole Oceanographic Institution, where the author was the Woods Hole Oceanographic Associates Lecturer for 1954, and part at the Institute for Advanced Study under contract N6ori Task Order I with the Office of Naval Research and the Geophysics Research Directorate, Air Force Cambridge Research Center. Reproduction of this article in whole or in part is permitted for any purpose by the United States Government.

[2] The conditions under which this assumption holds will be given in a forthcoming article by Veronis and Stommel (1956). There it is shown that, under ordinary circumstances, negligible inertio-gravitational oscillations of large scale are created by the atmospheric wind systems.

The exclusion of inertio-gravitational oscillations greatly simplifies the mathematical problem and permits analysis of more complicated systems. The formalism will be applied to the study of currents generated in an infinite double-layer ocean by the action of various distributions of wind stress. In part, some of Rossby's earlier findings will be substantiated; but, in illustration of the generality of the method, the formalism will be applied also to the case of an ocean with parallel boundaries to show the necessity, under certain conditions, for development of a narrow intense current near the boundaries with roughly the dimensions of the Gulf Stream. Finally, some attempt will be made to take into account the dynamical effects of the variation of the Coriolis parameter.

2. *The Balance Equations.* The relevant equations will be derived in somewhat more generality than is necessary for present use in order to bring out more fully the underlying assumptions. To do so, one must draw a fundamental distinction between the two types of large-scale motion that exist in the ocean. The two have widely differing time scales and interact only weakly with one another. They may be described broadly as "waves" and "currents," if one includes in the "wave" category not only surface waves but large-scale gravitational oscillations influenced by the earth's rotation, and, in the "current" category, what is ordinarily meant by transient or stationary ocean currents.

In order to formulate mathematically the distinction between the two categories, we define certain parameters that characterize the space and time scales of a given fluid motion. Let S be a characteristic horizontal dimension, U a characteristic horizontal particle velocity, and let Ω be the speed of the earth's rotation. Then the order of the horizontal acceleration per unit mass is U^2/S and the order of the Coriolis acceleration is ΩU. The nondimensional ratio of the two, $U/\Omega S$, will be called the Rossby number Ro, after Fultz (1951). If it is small, the flow is quasi-geostrophic. In this case one may show, by evaluating the order of the terms in the equation for the vertical vorticity component, that the ratio of horizontal divergence to vertical vorticity has the order Ro and is therefore small also. However, the essential part of the argument is that the divergence-vorticity ratio is small whether or not the flow is quasi-geostrophic, providing only that it is not a "wave" motion. In the case where the flow is nongeostrophic, we define H as a characteristic vertical dimension, $\Delta\rho$ as a characteristic vertical potential density difference, and (C, U) as the larger in magnitude of the phase or particle speeds, C or U. Then it may be shown that for nongeostrophic but quasi-hydrostatic motions the divergence-vorticity ratio becomes the reciprocal of the

intrinsic Richardson number, $R_i = g\,(\Delta\rho/\rho)\,H \div (C,\ U)^2$, which measures the ratio of the square of the speed of long internal gravitational waves to $(C,\ U)^2$.[3] The significance of R_i is that it is of order unity or smaller for gravity "waves" but is large compared to unity for "currents." Consequently one may state as a general rule that the divergence-vorticity ratio is small for "currents" and for "currents" only. Although exceptions to this rule exist, they are rare and somewhat pathological.

From what has been said, the current motions are to be considered horizontally quasi-nondivergent. This is not to say, however, that they are altogether nondivergent; small horizontal divergences must occur in conjunction with transverse horizontal and vertical displacements in order to bring about the instantaneous mass adjustment. Thus we may write the horizontal velocity vector $\vec{V}$ as the sum of a nondivergent and an irrotational vector:

$$\vec{V} = \nabla\psi \times \vec{k} + \nabla\alpha \equiv \vec{V}_\psi + \vec{V}_\alpha \quad , \tag{1}$$

where ψ is the stream function for the nondivergent component of the motion, α the velocity potential for the irrotational component, $\vec{k}$ a unit vertical vector, and ∇ the horizontal gradient operator; and we assume that $\vec{k} \cdot \nabla \times \vec{V} \equiv \vec{k} \cdot \nabla \times \vec{V}_\psi \gg \nabla \cdot \vec{V} \equiv \nabla \cdot \vec{V}_\alpha$ but that $\nabla \cdot \vec{V}_\alpha$ is not to be ignored. With suitable boundary conditions on ψ and α, one may deduce the important relation $|\vec{V}_\psi| \gg |\vec{V}_\alpha|$.

The formal system of equations may now be derived as follows. We assume that the flow is incompressible and that the horizontal scale is large in comparison to the vertical scale. The motion is then quasi-hydrostatic, and the Eulerian equations become

$$\bar{\rho}\left(\frac{\partial \vec{V}}{\partial t} + \vec{V} \cdot \nabla \vec{V} + w\frac{\partial \vec{V}}{\partial z} + f\vec{k} \times \vec{V}\right) = -\nabla p + \bar{\rho}\vec{F} \quad , \tag{2}$$

$$\rho g = -\frac{\partial p}{\partial z} \quad , \tag{3}$$

$$\nabla \cdot \vec{V} + \frac{\partial w}{\partial z} = 0 \quad , \tag{4}$$

$$\frac{\partial \rho}{\partial t} + \vec{V} \cdot \nabla \rho + w\frac{\partial \rho}{\partial z} = 0 \quad . \tag{5}$$

[3] This relationship may be derived for simple linear systems by analytic means, and for general systems by a kind of scale analysis similar to that used in the theory of the boundary-layer in fluid dynamics (Charney, 1948).

Here z is the vertical co-ordinate, t time, p pressure, ρ density, w vertical velocity, f the Coriolis parameter $2\ \Omega$ sin (latitude), and $\vec{F}$ the external force. For simplicity, the purely kinematic effects of the earth's curvature have been ignored and, as a quite valid approximation in the oceans as well as in the atmosphere, the density $\rho\ (x, y, z)$ in eq. (2) has been replaced by the standard value $\bar{\rho}\ (z)$.

We now replace the first vector equation by the two scalar equations obtained by taking its curl and divergence. The curl operation gives the vorticity equation

$$\left(\frac{\partial}{\partial t} + \vec{V} \cdot \nabla + w \frac{\partial}{\partial z}\right) Z = -\, Z \nabla \cdot \vec{V}_a - \vec{k} \cdot \nabla w \times \frac{\partial \vec{V}}{\partial z} + \vec{k} \cdot \nabla \times \vec{F} \quad , \tag{6}$$

in which Z is the vertical component of absolute vorticity,

$$Z = \vec{k} \cdot \nabla \times \vec{V}_\psi + f \equiv \nabla^2 \psi + f \quad . \tag{7}$$

The divergence operation gives

$$\left(\frac{\partial}{\partial t} + \vec{V} \cdot \nabla + w \frac{\partial}{\partial z}\right) \nabla \cdot \vec{V} + \nabla w \cdot \frac{\partial \vec{V}}{\partial z} + \left(\frac{\partial u}{\partial x}\right)^2 + 2 \frac{\partial u}{\partial y} \frac{\partial v}{\partial x} + \left(\frac{\partial v}{\partial y}\right)^2 - f \vec{k} \cdot \nabla \times \vec{V} - \vec{k} \cdot \vec{V} \times \nabla f = -\frac{1}{\bar{\rho}} \nabla^2 p + \nabla \cdot \vec{F} \quad ,$$

if x and y are respectively the eastwardly and northwardly directed horizontal cartesian co-ordinates and if u and v are the corresponding velocity components. Here we utilize the inequalities $\vec{k} \cdot \nabla \times \vec{V} \gg \nabla \cdot \vec{V}$ and $|\vec{V}_\psi| \gg |\vec{V}_a|$ in conjunction with the continuity eq. (4) to justify ignoring the first two terms on the left-hand side. Anticipating that $\vec{F}$ is the force of friction, which is important only in the layer of frictional influence, whose dynamics will be given special treatment in the next section, we may ignore it here. We then obtain the reduced form

$$f \nabla^2 \psi + \nabla \psi \cdot \nabla f + 2 \left[\frac{\partial^2 \psi}{\partial x^2} \frac{\partial^2 \psi}{\partial y^2} - \left(\frac{\partial^2 \psi}{\partial x \partial y}\right)^2\right] = \frac{1}{\bar{\rho}} \nabla^2 p \quad , \tag{8}$$

which states a direct relationship between the field of motion (expressed by the stream-function ψ) and the field of pressure. This is the *condition of balance.* If $\vec{V}$ is expressed in terms of ψ and α by (1), eqs. (3–8) suffice to determine the five dependent variables p, ρ, ψ, α, and w. We call them the *balance equations.* Essentially, the only change has been the replacement of the horizontal divergence equation by the balance condition (8).

The balance condition has been obtained independently by Fjortoft (unpublished work) as a necessary condition that the nondivergent part of the motion be stable with respect to horizontally divergent perturbations. We know of course that the oceanic motions *are* stable for the most part, for otherwise one should expect a spontaneous generation of "wave" motions from "current" motions.

The balance condition may be regarded as a generalization of the geostrophic approximation. If the inertial forces are small compared with the Coriolis force, the nonlinear term in ψ drops out and the resulting equation

$$f\nabla^2\psi + \nabla\psi \cdot \nabla f = \frac{1}{\bar{\rho}}\nabla^2 p$$

may be integrated to give

$$f\nabla\psi = \vec{V}_\psi \times \vec{k} = \frac{1}{\bar{\rho}}\nabla p \quad .$$

Solving for $\vec{V}_\psi$, we obtain

$$\vec{V}_\psi \cong \vec{V}_g \equiv \frac{\vec{k}}{\bar{\rho}f} \times \nabla p \cong k \times \nabla\left(\frac{p}{\bar{\rho}f}\right) \quad , \tag{9}$$

which is simply the geostrophic approximation.

If the intrinsic Rossby number is small everywhere, the quasi-geostrophic equations may be further simplified. The terms involving w in eq. (6) become small and may be ignored; the undifferentiated Z may be replaced by f; and $\vec{V}$ may be replaced in equations (5) and (6) by $\vec{V}_g$. If w is then eliminated between eqs. (4), (5) and (6), one obtains

$$\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right) Z_g + f\frac{\partial}{\partial z}\left[\left(\frac{\partial \rho}{\partial z}\right)^{-1}\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right)\rho\right] = \vec{k} \cdot \nabla \times \vec{F} \, , \tag{10}$$

which, when ρ and $\vec{V}_g$ are expressed in terms of p by means of (3) and (9), is alone sufficient to determine the motion.

3. *Specialization of Quasi-geostrophic Equations to Single- and Double-layer Oceans.* We shall consider oceans consisting of one or two homogeneous layers. If the flow is quasi-geostrophic as well as quasi-hydrostatic, it can be shown that the motion must be independent of height in each layer. The quasi-geostrophic equations may then be specialized in the following manner: The continuity eq. (4) is first integrated to give

$$\frac{\partial h}{\partial t} + \vec{V}_g \cdot \nabla h = -h\nabla \cdot \vec{V}_a = -h\nabla^2\alpha \quad , \tag{11}$$

where h is the thickness of the layer. We next substitute in (6) and use the geostrophic approximation $\vec{V}_\psi \cong \vec{V}_g$. Redefining the averaged force $\frac{1}{h}\int_0^h \vec{F}\, dz$ by $\vec{F}$, we obtain, after some combination of terms,

$$\left[\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right]\frac{Z_g}{h} = \frac{1}{h}\vec{k} \cdot \nabla \times \vec{F}. \tag{12}$$

In a single-layer homogeneous ocean with a free surface, pressure is related to thickness through the hydrostatic equation

$$p = \rho\, g(h - z) + \text{constant}. \tag{13}$$

Hence $\vec{V}_g = (g/f)\,\vec{k} \times \nabla h$, $Z_g = (g/f)\nabla^2 h + f$, and eqs. (11) and (12) suffice to determine the two dependent variables h and α. We note that if $\vec{F} = 0$, eq. (12) asserts the conservation of the "potential vorticity" Z_g/h.

In the two-layer case, let $\vec{V}$, h, ρ and p be the horizontal velocity, depth, density, and pressure respectively of the upper layer and denote corresponding quantities in the lower layer by primes. The hydrostatic conditions become

$$\begin{aligned} p &= g\rho(h + h' - z) + \text{constant} \\ p' &= g\rho'(\varepsilon h + h' - z) + \text{constant} \quad , \end{aligned} \tag{14}$$

where ε is the density ratio ρ/ρ'; the geostrophic velocities and vorticities are defined by

$$\begin{aligned} V_g &= (g/f)\,\vec{k} \times \nabla\,(h + h') \\ Z_g &= (g/f)\,\nabla^2\,(h + h') + f \end{aligned} \tag{15}$$

$$\begin{aligned} V_g' &= (g/f)\,\vec{k} \times \nabla\,(\varepsilon h + h') \\ Z_g' &= (g/f)\,\nabla^2\,(\varepsilon h + h') + f \quad . \end{aligned} \tag{16}$$

Eqs. (15) and (16) combined with (11) and (12), written for both primed and unprimed quantities, are then the governing set of equations.

4. *The Wind Force.* When the wind begins to blow over the surface of the ocean, momentum is transferred through the surface into deeper water by a complex frictional process. If the wind stress is uniform horizontally, then, as Ekman (1905) showed, the frictional force varies in magnitude and direction with depth, and the water particles oscillate inertially with varying phase and amplitude. On the other hand, if the wind is not uniform, or if rigid boundaries exist, the frictionally driven currents are divergent and produce pressure changes which lead to the establishment of permanent currents. Since we are not concerned here with such inertial oscillations as might exist—their energy will be small in any case—we must consider the effect of wind stress on the balanced part of the flow. This effect may be incorporated into the balance equations in the following manner:

Consider a continuously stratified ocean. If the surface wind stress is horizontally nonuniform, the horizontal divergence of the wind drift current within the layer of frictional influence far exceeds the divergence of the balanced part of the flow. Hence one may, with good approximation, attribute all divergence to the wind drift current. To this divergence corresponds a difference in vertical velocity along the vertical between the surface and the depth at which frictional influences become negligible. On the assumption that this depth D is small compared with the vertical scale of the balanced currents, the frictional divergence may be introduced as a boundary effect in the balance equations for the continuously stratified ocean. To show this, let the vertical velocity at the surface be w_0 and the vertical velocity at depth D be w_1. Then, if $\vec{V}_f$ is the drift current, we have

$$w_1 - w_0 = \int_{-D}^{0} \nabla \cdot \vec{V}_f \, dz \quad .$$

Ignoring inertial oscillations we may assume that the total Coriolis force acting on the drift current is exactly balanced by the surface stress $\vec{\tau}$, i.e., $\int_{-D}^{0} f \vec{k} \times \vec{V}_f \rho dz = \vec{\tau}$. In this formula the vertical variation of ρ may be ignored and one may divide both sides by ρf. Doing so and taking the curl of both sides, we obtain

$$\int_{-D}^{0} \nabla \cdot \vec{V}_f \, dz = \vec{k} \cdot \nabla \times (\vec{\tau}/\rho f) \quad .$$

The vertical velocity w_0 at the surface is the individual derivative of the height of the free surface ζ. Since depth D is relatively small, we may ignore the variations of density in the friction layer and write $d\zeta = dp_1/\rho_1 g$, where $_1$ denotes quantities at depth D. Hence

$$w_o = \frac{1}{\rho_1 g}\frac{dp_1}{dt} \equiv \frac{1}{\rho_1 g}\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right) p_1 \quad .$$

The vertical velocity w_1 at level D is given by eq. (5) with $\vec{V}$ evaluated geostrophically. Solving for w_1 we get

$$w_1 = -\left(\frac{\partial \rho}{\partial z}\right)_1^{-1}\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right)\rho_1 = \frac{1}{g}\left(\frac{\partial \rho}{\partial z}\right)_1^{-1}\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right)\left(\frac{\partial p}{\partial z}\right)_1 \quad .$$

Combining the last four equations, we obtain the required condition

$$\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right)\left(\frac{\partial p}{\partial z}\right)_1 + m\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right) p_1 = g\, m \vec{k} \cdot \nabla \times \left(\frac{\vec{\tau}}{f}\right) \quad ,$$

where $m = -(\rho^{-1}\, \partial\rho/\partial z)_1$. Since D is small, this equation may be assumed to apply approximately at the surface. The usefulness of this expression lies in the fact that it converts the complicated Ekman-spiral microstructure of the frictional layer into a simple boundary condition on pressure.

In the case where wind-induced friction acts in the upper of two homogeneous layers, the effect may be directly incorporated by subtracting the integrated frictional divergence through the entire upper layer from the individual depth change of this layer due to divergence of the balanced flow. Thus in eq. (11) we merely subtract the term $\vec{k} \cdot \nabla \times (\vec{\tau}/\rho f)$ from the left-hand side:

$$\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right) h = -h\nabla \cdot \vec{V} - \vec{k} \cdot \nabla \times \left(\frac{\vec{\tau}}{\rho f}\right) \quad .$$

Proceeding as before in the derivation of eq. (12), we obtain

$$\left(\frac{\partial}{\partial t} + \vec{V}_g \cdot \nabla\right)\frac{Z_g}{h} = \frac{Z_g}{h}\vec{k} \cdot \nabla \times \left(\frac{\vec{\tau}}{\rho f}\right) \quad ,$$

from which it follows that the body force $\vec{F}$ in eq. (12) may be identified with $\vec{\tau}/\rho h$, providing only that one may ignore the variations of h and f on the right-hand side of the above equation and that Z_g may be replaced by f. The latter approximation is always valid under quasi-geostrophic conditions, and the former is also permissible when one considers how little is known about actual surface stress distributions.

The above method of incorporating wind stress into the balance equations is similar to a method used by Charney and Eliassen (1949) for dealing with the effects of surface friction on atmospheric motions. By introducing the wind stress distribution as a boundary condition on pressure, or, in the case of the homogeneous upper layer, as a body force, it is possible to investigate the large-scale features of the wind-driven circulation without including in complete detail all the small-scale complexities of the surface frictional layer.

5. *Generation of Wind Currents in a Single-layer Ocean.* Now consider an infinite homogeneous ocean of constant depth H which is initially at rest. Beginning at time $t = 0$, a certain amount of y-momentum is imparted uniformly to an infinite strip of width 2a. What is the final equilibrium state? Rossby (1938) considered the case where the momentum is added impulsively. In the present analysis we allow the wind stress a finite time in which to act and we assume that the current is constantly in a state of quasi-geostrophic equilibrium. On the assumption that the motions are small, the appropriate equation for dealing with this situation is the linearized form of (12). Ignoring effects from the variation of f with latitude, we have simply

$$\left(\frac{\partial^2}{\partial x^2} - \frac{1}{\lambda^2}\right)\frac{\partial \eta}{\partial t} = \frac{f}{g}\frac{\partial F}{\partial x} \quad , \quad F = \begin{cases} B \text{ for } |x| \leq a \\ 0 \text{ for } |x| > a \end{cases} , \tag{17}$$

where η is the perturbation height of the ocean and λ is Rossby's "radius of deformation" $(gH/f^2)^{\frac{1}{2}}$. The solution of this equation, subject to the conditions that $\eta = 0$ at $t = 0$ and that η be finite at $x = \infty$, is found to be

$$\eta = \frac{\lambda f B t}{g} \begin{cases} e^{-a/\lambda} \sinh (x/\lambda) & , \quad |x| \leq a \\ \text{sign } x e^{-|x|/\lambda} \sinh (a/\lambda) & , \quad |x| > a \end{cases} \tag{18}$$

and is represented schematically in Fig. 1.

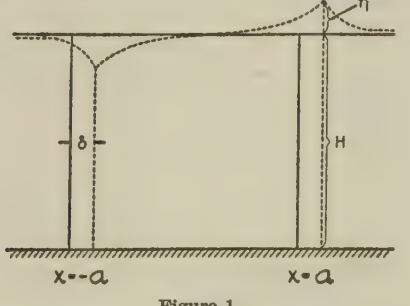

Figure 1.

The (infinitesimal) displacement δ of the central current to the right may be calculated from the transverse velocity u at $x = \pm a$. This is obtained from the continuity eq. (11) which, in the present instance, is

$$\frac{\partial \eta}{\partial t} = -H\frac{\partial u}{\partial x} \quad . \tag{19}$$

We find that $u(\pm a) = (B/2f)\,[1 + \exp(-2a/\lambda)]$. Since this quantity is independent of time, δ is merely $u(\pm a)t$. The velocity of the balanced current is obtained from $v = (g/f)\partial\eta/\partial x$,

$$v = Bt \begin{cases} e^{-a/\lambda}\cosh(x/\lambda) & , \quad |x| \leq a \\ -e^{-|x|/\lambda}\sinh(a/\lambda) & , \quad |x| > a \end{cases} \quad . \tag{20}$$

If the force B and the time during which it acts are varied in such a way that Bt remains constant, the resulting motion remains unchanged, since that part of the motion which depends on the rate at which momentum is added (the inertial oscillation) has been ignored. The final equilibrium state corresponding to a given addition of momentum is essentially the same as that in Rossby's model. The principal difference is that the energy imparted is here less by just the amount that is radiated away as gravitational wave energy in Rossby's model.

6. *Generation of Wind Currents in a Double-layer Ocean.* Of greater interest is the manner in which currents are created in a stratified ocean where there is the possibility of internal mass adjustment. Here we simplify the analysis by replacing the continuously stratified ocean by two homogeneous layers with the surface of discontinuity at the center of the mean thermocline as in Fig. 2.

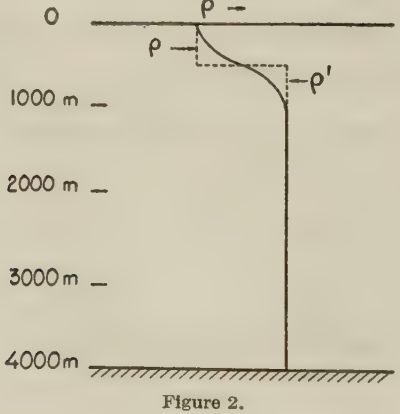

Figure 2.

To simulate actual conditions in the North Atlantic, we take the depths of the upper and lower layers to be 500m and 3500m respectively and the density difference $\rho' - \rho$ to be 2×10^{-3}gm cm^{-3}. Again, assuming small motions, we may disregard second order terms; thus eqs. (12), (15) and (16) may be combined to give

$$\begin{cases} \nabla^2 \dfrac{\partial}{\partial t}(\eta + \eta') - \dfrac{1}{\lambda^2}\dfrac{\partial \eta}{\partial t} + \beta \dfrac{\partial}{\partial x}(\eta + \eta') = \dfrac{f}{g}\vec{k} \cdot \nabla \times \vec{F} \quad , \\ \nabla^2 \dfrac{\partial}{\partial t}(\varepsilon\eta + \eta') - \dfrac{1}{\lambda'^2}\dfrac{\partial \eta'}{\partial t} + \beta \dfrac{\partial}{\partial x}(\varepsilon\eta + \eta') = \dfrac{f}{g}\vec{k} \cdot \nabla \times \vec{F}' \quad , \end{cases} \tag{21}$$

where η and η' are the perturbations of depths h and h' respectively, $\lambda = (gH/f^2)^{\frac{1}{2}}$ and $\lambda' = (gH'/f^2)^{\frac{1}{2}}$ are the radii of deformation corresponding to the upper and lower depths H and H', and $\beta = df/dy$. For simplicity, β is assumed constant.

In accordance with the idea that most of the frictional force acts in the upper hundred meters or so, we assume that no force acts in the lower layer and that there is no frictional transfer of momentum across the interface. Any motion created in the lower layer must therefore occur in response to pressure forces transmitted from above.

It will be convenient to separate the motion into its two normal modes: a barotropic, or external, mode in which the current velocities in both layers are the same and a baroclinic, or internal, mode in which the current velocities in each layer are inversely proportional to their depths. This separation is performed as follows: We multiply the first of eqs. (21) by σ and add to the second, choosing σ so that the linear combinations $\sigma(\eta + \eta') + \varepsilon\eta + \eta'$ and $\sigma\eta/\lambda^2 + \eta'/\lambda'^2$ are proportional. Denoting the ratio H/H' by r, we find that σ must satisfy the equation

$$\sigma^2 + (1 - r)\sigma - r\varepsilon = 0 \tag{22}$$

whose roots are

$$\sigma_1, \sigma_2 = -(1 - r)/2 \pm [(1 + r)^2/4 - r(1 - \varepsilon)]^{\frac{1}{2}}.$$

Since $(1 - \varepsilon) = (\rho' - \rho)/\rho' = 2 \times 10^{-3}$, whereas $(1 + r)^2/4 > \frac{1}{4}$, we may approximate these roots by

$$\begin{aligned} \sigma_1, \sigma_2 &= -\frac{(1 - r)}{2} \pm \frac{(1 + r)}{2}\left[1 - \frac{2r(1 - \varepsilon)}{(1 + r)^2}\right] \\ &= r - \frac{(1 - \varepsilon)r}{1 + r} \quad , \quad -1 + \frac{(1 - \varepsilon)r}{1 + r}. \end{aligned} \tag{23}$$

Defining

$$R_i = \sigma_i(\eta + \eta') + \varepsilon\eta + \eta' \ (i = 1, 2) \quad , \tag{24}$$

we obtain the required decomposition,

$$\nabla^2 \frac{\partial R_i}{\partial t} - \frac{\alpha_i^2}{\lambda^2} \frac{\partial R_i}{\partial t} + \beta \frac{\partial R_i}{\partial x} = \sigma_i \frac{f}{g} \vec{k} \cdot \nabla \times \vec{F} \quad , \tag{25}$$

where

$$\begin{cases} \alpha_1^2 = \dfrac{1}{1-\varepsilon}\left(1 - \dfrac{r\varepsilon}{\sigma_1}\right) \approx \dfrac{r}{1+r} \quad , \\ \alpha_2^2 = \dfrac{1}{1-\varepsilon}\left(1 - \dfrac{r\varepsilon}{\sigma_2}\right) \approx \dfrac{1+r}{1-\varepsilon}. \end{cases} \tag{26}$$

Once the R_1 have been found, $\eta + \eta'$ and $\varepsilon\eta + \eta'$ may be obtained by solving (24). Thus

$$\begin{cases} \eta + \eta' = \dfrac{R_1 - R_2}{\sigma_1 - \sigma_2} \approx \dfrac{R_1 - R_2}{1+r} \quad , \\ \varepsilon\eta + \eta' = \dfrac{\sigma_1 R_2 - \sigma_2 R_1}{\sigma_1 - \sigma_2} \approx \dfrac{R_1 - rR_2}{1+\mathrm{r}}. \end{cases} \tag{27}$$

It is evident that R_1 corresponds to the barotropic mode, since, for this component of motion, the geostrophic velocities $(g/f)\,\vec{k} \times \nabla\,(\eta + \eta')$ and $(g/f)\,\vec{k} \times \nabla\,(\varepsilon\eta + \eta')$ are equal. Likewise we see that the upper and lower velocities corresponding to R_2 are in the ratio $1/r$. This mode will be called the baroclinic, or internal, mode and is characterized by the large deformations of the interface needed to reduce the pressure gradient in the lower layer. The slope of the interface is of the order of one thousand times that of the free surface.

Let us assume now that a uniform wind stress acts on the infinite strip, $|x| \leq a$, producing a body force B in the y-direction and acting only in the upper layer. Disregarding the β-effect, we find, in complete analogy with the single layer case,

$$R_i = \frac{\sigma_i \lambda f B t}{\alpha_i g} \begin{cases} e^{-\alpha_i a/\lambda} \sinh(\alpha_i x/\lambda) & , \quad |x| \leq a \quad , \\ \operatorname{sign} x\; e^{-\alpha_i |x|/\lambda} \sinh(\alpha_i a/\lambda) & , \quad |x| > a \quad . \end{cases} \tag{28}$$

The corresponding y-velocity components are

$$v_i = \frac{g}{f} \frac{\partial R_i}{\partial x} = \sigma_i B t \begin{cases} e^{-\alpha_i a/\lambda} \cosh(\alpha_i x/\lambda) & , \quad |x| \leq a \\ -e^{-\alpha_i |x|/\lambda} \sinh(\alpha_i a/\lambda) & , \quad |x| > a \quad . \end{cases} \tag{29}$$

The upper layer velocities corresponding to each of the modes R_1 and R_2 are shown in Fig. 3 for the half-widths $a = 100$km and $a = 1000$km. It is seen that the baroclinic mode is predominant over a large part of

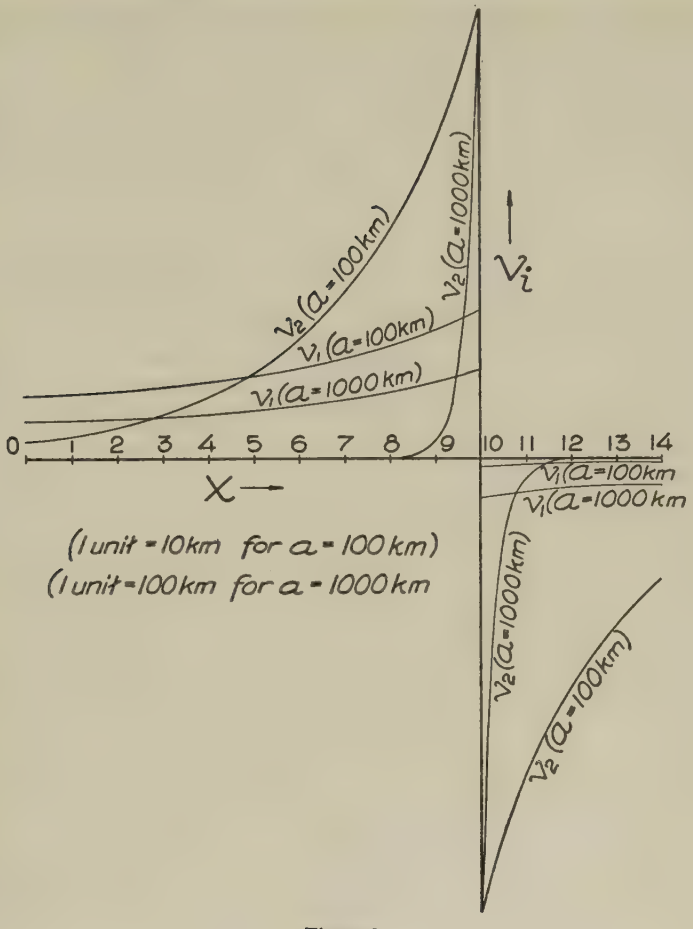

Figure 3

the current region for the smaller width whereas for the larger width the barotropic mode dominates except near the edges.

To estimate the orders of magnitude of the currents generated, we assume that the surface stress τ is one dyne cm^{-2} and f is 10^{-4} sec^{-1}. The body force then becomes $B = \tau/\rho H = 1.73$ cm sec^{-1} per day. The maximum currents and countercurrents occur at the edges. After one year, max $v_1 = 0.9$ m sec^{-1} for $a = 100$km and 0.6 m sec^{-1} for $a = 1000$km and max $v_2 = 3.2$ m sec^{-1} for either $a = 100$km or $a = 1000$km.

To eliminate the edge effect, an effect which depends on an unrealistic wind stress distribution for the open sea, we replace the step-function force by a sinusoidally varying one, $F = B \sin kx \vec{j}$, where $\vec{j}$ is a unit vector in the y-direction. The solution to eq. (27), again without the β-term, then becomes, in terms of $v_i = (g/f)\ (\partial R_i/\partial x)$,

$$v_i = \sigma_i\,(1 + \alpha_i^2/\lambda^2 k^2)^{-1}\, Bt \sin kx\ . \tag{30}$$

Since $\sigma_1 \approx r = 1/7$ and $\sigma_2 \approx -1$, the ratio of the amplitude of the barotropic to that of the baroclinic mode depends primarily on the nondimensional quantities $\alpha_i^2/\lambda^2 k^2$. The baroclinic mode becomes important only when $\alpha_2^2/\lambda^2 k^2 \lesssim 1$, i.e., when the wave-length $L = 2\pi/k$ is of the order of 100km or less. This result agrees with Rossby's conclusions that under ordinary circumstances the internal adjustment of the interface due to transverse circulations is never sufficient to cancel the lower current.

An explanation of this phenomenon may be sought in terms of the stability of the circular vortex, of which the above treated parallel currents constitute a special case. The geostrophically balanced component of flow may be regarded as the motion of a stationary circular vortex at a large distance from the axis of rotation. The transverse, nongeostrophic, circulation then corresponds to the meridional forced circulation caused by addition of angular momentum to the zonal flow. Eliassen (1952) has taken this view in his analysis of the response of a baroclinic circular vortex to radially symmetrical injections of angular momentum or heat. He also assumes that the zonal flow is at all times balanced so that meridional circulations are necessary in order to maintain the balance. He finds that the meridional motion then takes place mainly in the direction of smallest stability. The stabilizing forces in the present instance are gravitational stability of the free surface and of the interface and inertial stability due to the earth's rotation. The first two act to resist vertical displacement and the last acts to resist horizontal displacements. As the lateral extent of the wind force (determined by a in the first problem and by L in the second) decreases, the vertical component of the transverse circulation increases relative to its horizontal component. Since gravity resists internal displacements of the interface much less than it resists displacements of the free surface, it is natural to expect the baroclinic modes to predominate for sufficiently small a or L.

Another explanation of this effect follows from the requirement that the angular momentum in the lower layer remain constant in the absence of external torques. Simplifying matters by supposing

that the slopes m of the free surface and m' of the interface are constant in the region $|x| \leq a$ where wind force B acts, we find that

$$\varepsilon m + (1 - \varepsilon)m' + (a^2/3\lambda'^2)m' = 0 .$$

This equation is derived from the condition that the increase in angular momentum associated with increase in velocity is exactly balanced by the decrease due to the transport of water toward the axis of rotation.

Solving for the ratio m/m', we find

$$\frac{m}{m'} = -(1 - \varepsilon)\left[1 - \frac{a^2}{3(1 - \varepsilon)\lambda'^2}\right] ,$$

which shows that m/m' is of the order $(1 - \varepsilon)$ and that the flow is baroclinic only if the ratio $a^2/(1 - \varepsilon)\lambda'^2$, corresponding to $\alpha_2^2/\lambda^2 k^2$ in the last example, is of the order 1 or less, a confirmation of our earlier result.

The curious development of the internal mode at the edge of the limited area over which a uniform wind is blowing may also be qualitatively explained by the above analysis. Near the edge, transverse horizontal displacements must give way to vertical displacements, and these are much more easily accommodated by a deformation of the interface than of the free surface.

While the sharply limited wind distribution is unrealistic for the open ocean, the effect it produces may itself be of the highest importance; for precisely such an effect must be expected near coasts where, as a result of wind-induced lateral circulations, water piles up or is removed, and where large deformations of the thermocline occur. One may speculate that the intense jet-like currents found on the western shores of the major oceans, such as the Gulf Stream and the Kuroshio, are due to a related phenomenon. We shall investigate this possibility further in the next section.

7. *The Coastal Jet.* Temperature profiles through the Gulf Stream (Worthington, 1953) reveal a marked baroclinicity. Profiles near 69° W, 38° N, measured in October, show an isotherm near the center of the thermocline sloping from say 800 m depth at the seaward edge of the Gulf Stream to nearly the surface at its center. It seems certain that a theory that would explain this phenomenon would also go far toward explaining the Gulf Stream itself; and it is difficult to avoid the conclusion that the theory must deal with an essentially baroclinic ocean. The most attractive of the existing theories, those of Stommel (1948) and Munk (1950), account for many of the *mean* features of the ocean circulations, including the mean westward

intensification, but since they deal only with a homogeneous ocean or with the total mass transport in a vertical column, they do not allow for effects of baroclinicity. Further, in Munk's theory, the detailed structure of the western current depends in an essential way on a lateral eddy viscosity whose magnitude requires eddies of the size of the Gulf Stream meanders themselves. Such a theory cannot be expected to apply to the individual jet.

The calculations to be presented below are not intended as a theory. They are meant to be suggestive only. However, they do have the advantage of referring to an actual, not a mean, jet-stream, and they take explicit account of the baroclinicity.

The system to be considered first is an infinite double-layer ocean confined between two parallel walls, $x = \pm a$. Assuming that the ocean is initially at rest, we inquire into the motion created by a wind blowing parallel to the walls and varying sinusoidally from one wall to the other according to the law $G = -B \sin(\pi x/2a)\vec{j}$. It should be emphasized that the system under consideration is essentially transient, since we postulate no dissipation of energy, without which no steady state can be reached. A complete analysis would contain a more realistic wind stress acting in an enclosed ocean and would include a dissipation mechanism. The point of view adopted here is that a mechanism which will bring the thermocline to the surface in the vicinity of coasts must continue to operate even after various modifying factors are brought into play.

If the β-effect is ignored, the equations of motion (25) take the form:

$$\left(\frac{\partial^2}{\partial x^2} - \frac{\alpha_i^2}{\lambda^2}\right)\frac{\partial \overline{R}_i}{\partial t} = -\sigma_i \cos kx \quad , \tag{31}$$

with $k \equiv \pi/2a$ and with $R_i = (kf\,B/g)\overline{R}_i$. The boundary condition is obtained by integrating the linearized continuity equations

$$\frac{\partial \eta}{\partial t} = -H\frac{\partial u}{\partial x} \quad , \quad \frac{\partial \eta'}{\partial t} = -H'\frac{\partial u'}{\partial x}$$

with respect to x. Since u and u' vanish at $x = \pm a$, we get

$$\frac{\partial}{\partial t}\int_{-a}^{a} \eta dx = \frac{\partial}{\partial t}\int_{-a}^{a} \eta' dx = 0 \quad ,$$

or, in terms of $\overline{R}_i$,

$$\frac{\partial}{\partial t}\int_{-a}^{a} \overline{R}_i dx = 0 \quad . \tag{32}$$

The solution of the system (31) and (32) is

$$\bar{R}_i = \frac{\sigma_i t}{k^2 + \alpha_i^2/\lambda^2}\left[\cos kx - \frac{\alpha_i}{\lambda k}\frac{\cosh(\alpha_i x/\lambda)}{\sinh(\alpha_i a/\lambda)}\right], \tag{33}$$

which gives, for the velocities of the normal modes,

$$v_i = \frac{g}{f}\frac{\partial R_i}{\partial x} = -\frac{\sigma_i k^2 Bt}{k^2 + \alpha_i^2/\lambda^2}\left[\sin kx + \frac{\alpha_i^2}{\lambda^2 k^2}\frac{\sinh(\alpha_i x/\lambda)}{\sinh(\alpha_i a/\lambda)}\right]. \tag{34}$$

The first term in parentheses corresponds to the directly forced motion, in phase with the wind force; the second term corresponds to the motion produced by the Coriolian divergence of water from the coasts. We note by comparison with eq. (29) that this latter effect is analogous to that already found in the case where the wind stress is uniform.

For the baroclinic mode, $\alpha_i^2 > > \lambda^2 k^2$ for widths comparable to ocean widths. Hence, near the coasts, $v_2 \approx \pm \sigma_2 Bte^{-\alpha_2(a-|x|)}$ and is very small elsewhere. The maximum velocity of the barotropic mode is $\sigma_1 Bt$ and diminishes slowly toward the center. At the coastal boundaries the velocity of the baroclinic mode is greater than $\sigma_1 Bt$ by the factor $-\frac{\sigma_2}{\sigma_1} \approx \frac{1}{r} = 7$. The distance from the coast beyond which the baroclinic velocity becomes small is $\lambda/\alpha_2 = \left[\frac{g(1-\varepsilon)H}{f^2(1+r)}\right]^{\frac{1}{2}}$, or about 30 km. If B corresponds to a wind stress of 1 dyne cm^{-2}, the upper layer velocity at each coast would become 2 m sec^{-1} after about four months. In this time the interface at the coast would rise 220 m above its undisturbed depth.

So far we have considered only cases where the wind blows parallel to the shores of an infinite ocean strip. If a wind begins to blow at right angles, say with a sinusoidal variation in the y-direction, no divergence *away* from the coasts will take place. Instead, piling up of water will occur in troughs and ridges at right angles to the coasts. Only later, as a consequence of pressure forces along the coast resulting from the initial divergence, will velocities parallel to the coasts be generated. In the absence of such asymmetries as might be caused by variation of the Coriolis parameter, this mechanism turns out to be not as effective as the parallel wind action in generating intense coastal currents.

To calculate the effect of a normal wind, we assume a wind force of the form $F = B\cos \mu y\, \vec{i}$, where $\vec{i}$ is a unit vector pointing westward, beginning to act at $t = 0$. Eq. (25) then becomes

$$\nabla^2 \frac{\partial R_i}{\partial t} - \frac{\alpha_i^2}{\lambda^2} \frac{\partial R_i}{\partial t} = - \frac{\sigma_i f \mu}{g} B \sin \mu y \,. \tag{35}$$

The vanishing of the normal velocity components u and u' at the boundaries is expressed by the geostrophic conditions

$$\frac{\partial R_i}{\partial y} = 0 \quad . \tag{36}$$

If $\gamma_i^2 = \mu^2 + \alpha_i^2/\lambda^2$ and $\omega_i = f\mu/\gamma_i$, the solution of the system (35) and (36) may be written

$$R_i = \frac{\sigma_i \mu f B t}{g \gamma_i^2} \left(1 - \frac{\cosh \gamma_i x}{\cosh \gamma_i a} \right) \sin \mu y \tag{37}$$

or, in terms of the y-velocities of the normal modes,

$$v_i = - \frac{\sigma_i \mu B t}{\gamma_i} \frac{\sinh \gamma_i x}{\cosh \gamma_i a} \sin \mu y \quad . \tag{38}$$

Taking $\mu = \dfrac{2\pi}{4000}$ km^{-1} we find for the ratio of baroclinic to barotropic velocity at a point on the coast where the wind force is a maximum

$$- \frac{\sigma_2}{\sigma_1} \frac{\gamma_1}{\gamma_2} \approx \frac{\mu \lambda}{r \alpha_2} \approx 0.37 \quad .$$

It would seem, therefore, that the wind stress must have components which are parallel to the coasts or which have a very large curl (large μ) if sharp coastal currents of appreciable strength are to be created.

8. *The Effect of the Variation of the Coriolis Parameter.* In the various examples treated, the barotropic (or external) mode of motion was found to be predominant except near coasts; that is to say, the current velocities in the open sea showed no tendency to decrease with depth, as they are observed to do. This result confirms the findings of Rossby, who likewise was unable to account dynamically for a rapid decrease of velocity with depth. However, Stommel has suggested that if one were to take into account the variation of the Coriolis parameter, this result might be altered. The following considerations will show that such indeed is the case if the ocean circulations are produced by transient wind systems moving with sufficiently large period. The physical explanation is that the variation of f permits self-propagating disturbances, and the impressed force system may approach resonance with the free baroclinic oscillatory mode.

To demonstrate this effect, we consider the forced oscillations produced by a traveling wind system exerting the force $F = B \sin (kx - \nu t) \sin \mu y \vec{j}$. The resulting motion, given by solution of eq. (25), is

$$R_i = \frac{\sigma_i f B}{\beta g} \frac{\sin (kx - \nu t)}{(1 - \nu/\nu_i)} \sin \mu y \quad , \tag{39}$$

where β is df/dy, and where

$$\nu_i = - \frac{k\beta}{k^2 + \mu^2 + \alpha_i^2/\lambda^2} \tag{40}$$

is the frequency of the Rossby wave corresponding to the ith mode. If $k = \frac{2\pi}{4000}$ km^{-1} and $\mu = \frac{\pi}{4000}$ km^{-1} we find for the periods $2\pi/\nu$: 9 days in the barotropic case and 8 years in the baroclinic case. Resonance occurs only if the wind system moves toward the west, an unusual occurrence. However, as $2\pi/\nu$ becomes of the order of or larger than the baroclinic period of 8 years, the baroclinic amplitude begins to predominate, and in the limit, $2\pi/\nu = 0$, it exceeds the barotropic by the factor $- \sigma_2/\sigma_1 \approx 7$.

If, instead of a forced disturbance, we consider the disturbance produced from rest by the sudden action of the stationary wind force $F = B \sin kx \sin \mu y \vec{j}$, we find that

$$R_i = \frac{\sigma_i f B}{\beta g} \left[\sin kx - \sin (kx - \nu_i t) \right] \sin \mu y \quad , \tag{41}$$

where the first term in the bracketed expression corresponds to the forced disturbance and the second term to the free disturbance. It is seen that, in both, the baroclinic mode is larger than the barotropic in the ratio $- \sigma_2/\sigma_1 \approx 1/r = 7$.

Unfortunately the foregoing results cannot be applied directly to the circulation produced in an enclosed ocean by a stationary wind field. It is not known how the resonance considerations become modified by the inclusion of boundaries. This problem is still under investigation. However, judging from the central role played by the planetary vorticity effect in Stommel's and Munk's theories of the wind-driven ocean circulation, it seems certain that any successful treatment of the planetary ocean circulation must eventually take it into account. Thus for example Stommel finds that the variation of the Coriolis parameter is responsible for a general westward intensification of the ocean circulation in a rectangular ocean. In the present case, where the circulation is produced from rest by wind action, one

should expect to find the same effect. But here it has been shown that a reduction of the horizontal extent of a current is accompanied by an intensification of its baroclinic component near the coast. Hence the β-effect combined with the development of the baroclinic mode should lead to a strengthening of the sharp coastal current on the western side and a weakening on the eastern side.

9. *Summary and Conclusion.* On the assumption that both stationary and transient ocean currents produced by the action of large-scale atmospheric wind systems are in a state of quasi-nondivergent balance between pressure and velocity, a system of equations has been deduced which has the virtue of applying only to current motions to the exclusion of background inertio-gravitational wave "noise." In these equations the cross-current circulations, needed to restore the pressure-velocity balance when it is disturbed by wind action, were treated as forced motions. A method has been given whereby the complex Ekman-spiral action of the wind-induced forces in the layer of frictional influence is replaced by a simple boundary condition in the case of a continuously stratified atmosphere or by a body force in the case of an ocean consisting of superimposed homogeneous layers. Application of the combined formalism to the behavior of a double-layer ocean under the influence of wind stresses has led to the result that, in the absence of effects due to the variation of the Coriolis parameter, uniform currents extending all the way to the ocean bottom are created. This result confirms some earlier work of Rossby.

There was, however, an important exception. In the immediate vicinity of coasts, large deformations of the interface were found to occur because of the action of gravitational stabilizing forces. These deformations counteract the establishment of deep currents in such a way that the flow is confined primarily to the upper thin-layer. The width of the resultant coastal currents is of the order of 30 km and therefore it corresponds roughly to the observed widths of the Gulf Stream and Kuroshio. In order that such currents may form, the wind must have either a component parallel to the shore or an unusually large curl. (This last result may be changed when the variation of the Coriolis parameter is taken into account, for it will have the effect of reducing the extent of the western current and thereby, as was shown previously, accentuating still further the narrow baroclinic coastal current.)

In accordance with a suggestion of Stommel, a preliminary examination was made of modifications caused by the variation in the Coriolis parameter. It was found that in open oceans the variation gives rise to currents that are confined mainly to the upper layer if the wind

stress system is horizontally nonuniform and moves very slowly, i.e., if it oscillates with a period of the order of or greater than 8 years. It has not been shown, however, to what extent these results carry over to enclosed oceans.

To account for the steady-state ocean circulation, one must evidently include a dissipation mechanism. The writer has attempted only to show how, in certain circumstances, the motion produced from rest by the action of a wind stress on the surface of a *baroclinic* ocean will generate intense coastal currents. Obviously the next step is to deal with an enclosed ocean with a variable Coriolis parameter and to take dissipation into account. However, even then it is not obvious how a dissipation mechanism will be introduced. There is some evidence that much of the total energy dissipation in the North Atlantic occurs as a result of Gulf Stream *meandering*. If this is so, then the circulation is intrinsically nonsteady and perhaps could best be approached as an initial value problem.

Finally, it should be pointed out that the explanation which has been given for the coastal currents has depended in an essential way on the transient character of the flow. One must postulate that after intense currents have been developed they will be maintained by mass advection in a manner that can be accounted for by only inclusion of nonlinear terms in the continuity and vorticity equations. One may surmise that mass transports produced by the action of wind stress in the open ocean will be successfully explained by a theory very close to that of Munk—perhaps involving some density stratification—and that the sharp coastal currents will be found to be a dynamic response of stratified coastal waters to these transports.

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DISCUSSIONS ON THE RELATIONSHIPS BETWEEN METEOROLOGY AND OCEANOGRAPHY

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G. E. R. DEACON:

There must be some of you who, like me, did most of their seagoing 20 to 30 years ago and find it difficult to keep pace with the present urgency of introducing more precise reasoning into oceanography. This afternoon's session dealing mainly with such precise arguments has not been easy to follow, and it is clear that there is much serious hard work ahead. Nevertheless the meeting seems to be ending on an optimistic and cheerful note. The signs of light relief remind me of the Promenade concerts held in the United Kingdom. When all of the serious work has been done, the last concert of the season deteriorates into a merry party in which the audience more or less takes charge and shows its enthusiasm by light-hearted co-operation with the orchestra; "Britannia rules the waves" is always the favourite song. I dare not hint at such a possibility either for sea waves or microseisms.

I should like to do what I can to emphasize the essential unity of the aims of oceanography with those of meteorology. In both there is growing emphasis on the study of general circulation and on the exchange of energy between the oceans and atmosphere, and there seems to be every reason why we should follow each other's work closely. This afternoon's papers have shown that when a meteorologist talks about circulation of air over the ocean he has figures for all the variables such as distribution of wind strength and angular

momentum, but, so far as I can judge, he does not have a very complete picture of the circulation of the air. He knows how fast everything goes but has no certain information of where the air comes from or where it is going. He still talks in terms of the pressure distribution which results from movements of air masses, but most of us, not carrying a specimen weather map in our heads, would rather be told in plain words that a current of cold air is approaching than be required to work it out for ourselves from what we are told about variations in pressure distribution.

The oceanographer has experienced the opposite approach. It is so easy for him to plot the horizontal and vertical distribution of temperature and salinity that he has a good idea of the origin and fate of different water masses. But, although he can show the complex interplay of subsurface currents in magnificent coloured diagrams, he has no idea how fast they go, and he is just as uncertain about the driving forces. For 100 years or more most oceanographers have attributed surface currents to the effect of wind, but, since it is not easy to see how the wind can energize deep-water movements, there has been a tendency to attribute deep currents to differences between the density of surface waters in different climatic areas. Twenty years ago I used to argue with Professor Sverdrup my conviction that such convective processes were all-important in deep layers of the ocean. I should be much more careful now, and he may have modified his ideas too. We have to recognize the possibility that the wind is the main driving force, even of deep currents; the fact that such currents appear at appropriate density levels in the ocean may indicate only that these are the paths of least resistance. Stommel has emphasized the weakness of our approach in his recent pamphlet "Why have our ideas on oceanic circulation such a dream-like character?" It is simply that we have not made sufficiently determined attempts to measure actual velocities of subsurface and deep currents.

Oceanography already owes much to meteorology, since most of our scientists who can deal with the theoretical complexities of the study of water movements on the rotating earth have been trained in schools of dynamic meteorology. We also seem to be learning from the measuring techniques used in meteorology. I have just taken part in attempts, which had a fair measure of success, to follow the equivalent of a radiosonde which sank slowly to the sea bed instead of ascending into the sky. W. V. R. Malkus has just made measurements by towing a current meter from a ship; this method seems more promising than that of making measurements from a ship anchored in deep water and is rather like using an aeroplane to find relative

wind speeds. Measurements are also being made by suspending large drogues on thin wires from small surface markers.

The meteorologists are not likely to gain much from oceanographic methods and techniques, but some of their theoretical conclusions may be more easily tested in the relatively sluggish ocean, and they need to know the part played by the ocean in heat and energy exchange and in the transport of heat from one region to another. There seems to be every reason for close co-operation.

H. U. SVERDRUP:

When the chairman approached me, I replied that I might say a few words about the transport of heat from lower to higher latitudes by ocean currents.

For the earth as a whole it is well known that in low latitudes the annual incoming radiation from the sun exceeds the outgoing terrestrial radiation, whereas in high latitudes the loss of heat by terrestrial radiation exceeds the gain by solar radiation. The total amounts received and lost by areas between every tenth parallel of latitude in the northern hemisphere are shown by curves *a* and *b* in Fig. 1. In order to maintain a constant average temperature in the different latitudes, the excess energy received south of about 36° N must be transported into higher latitudes to compensate for the loss in these areas. The necessary transport of heat across parallels of latitude is shown by curve *c* in Fig. 1.

It is frequently assumed that this transport is accomplished by atmospheric circulation, but it is obvious that in the northern hemisphere the ocean currents must also play a part. Warm water is transported to the north in the western parts of the oceans and cold water flows south in the eastern parts, meaning that there is a net flow of heat to the north. From a knowledge of the water temperature and of the current direction and velocity at all depths it should be possible to compute this heat transport, but so far only a single estimate seems to have been made. According to this the transport of heat by ocean currents across 55° N in the Atlantic Ocean amounts to about 0.3×10^{-6} g cal min^{-1}, which is about 7% of the total transport across that parallel.[1] Although the transport by current in lower latitudes has not been examined by direct calculation, it is possible to obtain some idea of the effect of ocean currents by using another method of approach.

[1] Sverdrup, H. U., M. W. Johnson, and R. H. Fleming, 1942, The Oceans. New York, Prentice-Hall, Inc. 1087 pp.

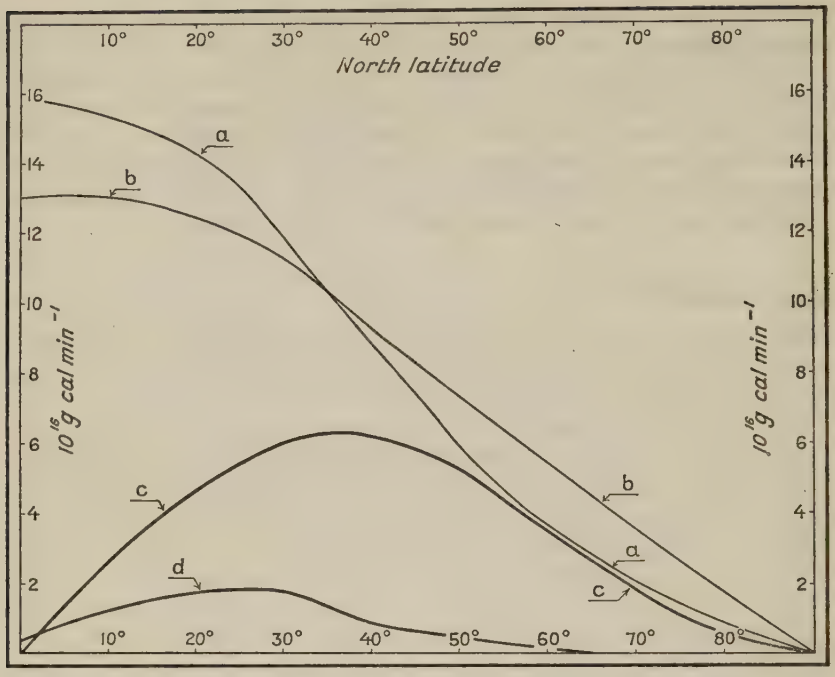

Figure 1. Curve *a:* Average annual amount of energy received by solar radiation between parallels of latitude that are 10° apart. Curve *b:* Average annual amount of energy lost by terrestrial radiation between parallels of latitude that are 10° apart. Curves *a* and *b* apply to the northern hemisphere (F. Baur and H. Philips, 1933, Beitr. Geophys., *45:* 82). Curve *c:* Average annual transport of energy to the north across parallels of latitude in the northern hemisphere. Curve *d:* Average annual transport of energy to the north across parallels of latitude by the ocean currents of the northern hemisphere.

We know that in all latitudes the oceans receive in one year more energy by solar radiation than they lose by terrestrial radiation and that the surplus is used for evaporation or is given off to the atmosphere as sensible heat. If there were no currents, the surplus of energy received by radiation, Q_r, would have to be used locally for evaporation (Q_e) or be given off as heat (Q_h):

$$Q_r = Q_e + Q_h \quad . \tag{1}$$

This simple equation must hold for the oceans as a whole, but, when dealing with a limited area, say an area between parallels of latitude, it must be borne in mind that heat may be transported in or out of that area by ocean currents. Calling this amount Q_v, we must have

$$Q_r = Q_e^1 + Q_h^1 - Q_v \quad , \tag{2}$$

where Q_e^1 and Q_h^1 now represent the amounts of energy that are used within the area for evaporation or are given off to the atmosphere as heat. Therefore,

$$Q_v = (Q_e^1 + Q_h^1) - Q_r \quad . \tag{3}$$

A negative value of Q_v indicates that heat is carried *out* of the area by currents.

Values of Q_e^1 and Q_h^1 have been derived by Jacobs[2] from meteorological observations, and values of Q_r have been computed by W. Schmidt and H. Mosby. Jacobs' computed values of $(Q_e^1 + Q_h^1)$ have been adjusted so that his averages for the oceans of the northern hemisphere equal $(Q_e + Q_h)$ of (eq. 1). Even if the values of Q need revision, this means that the difference (Q_v) between revised values of Q_r and correspondingly revised values of $(Q_e^1 + Q_h^1)$ need not be greatly influenced.

The numerical values have been published elsewhere,[3] but here only the results are represented by curve *d* in Fig. 1, which shows the transport of heat across the northern hemisphere. Note that a transport across the equator has been added because there warm surface water flows north along the coast of South America whereas cold deep water flows south.

Evidently the transport by ocean currents is not negligible, particularly to the south of 30° N where it accounts for more than 30% of the total transport. The values apply to the combined effect of currents in the North Pacific and North Atlantic Oceans.

Corresponding values for the southern oceans are not available, but in the southern hemisphere a smaller effect of the ocean currents must be expected. This expectation is substantiated by observations of evaporation from pans aboard ships, which have been discussed by Wüst.[4] In the northern hemisphere the observed values of Q_e^1 agree fairly well with values computed by Jacobs and deviate insignificantly from Q_e, but in the southern hemisphere no such systematic deviations occur, indicating that there the term Q_v is small.

If these results are correct, it must be concluded that the atmosphere transports significantly less heat from lower to higher latitudes in the northern hemisphere than it transports in the southern hemisphere; therefore, the atmospheric circulation within the two hemispheres must show certain characteristic differences.

[2] Jacobs, W. C., 1942, On the energy exchange between sea and atmosphere. J. Mar. Res., *5:* 37–66.

[3] Sverdrup, H. U., 1951, Compendium of Meteorology, Amer. met. Soc., 1071–1081, Boston.

[4] Wüst, G., 1930, Veröff. Inst. Meeresk. Univ. Berl.. N. F. (A) Heft 6, 95 pp.

HENRY STOMMEL:

I am asked to discuss the papers just delivered by Drs. Palmén and Charney and to consider in general the relation between oceanography and meteorology. I believe I can do this most effectively by a comparison concerning the present status of meteorological and oceanographical science.

It is not an exaggeration to state that efforts in oceanography for the past three-quarters of a century have revealed essentially a climatological mean distribution of temperature and salinity, and possibly a distribution of surface current velocity but certainly not of the velocities involved in the deep circulation of oceans.

A meteorologist might be appalled to realize that we still have no mean current charts other than those for the very surface. Due to technical difficulties, the distribution of currents in deep water has not been directly observable. We don't even know how to make such observations, and therefore we must exercise our technical ingenuity to measure deep current velocities. Yesterday Dr. Deacon described to me briefly a sonic technique which his laboratory is now using for measurements of weak currents to a depth of 1000 fathoms. I hope he will take the opportunity of this discussion to describe the nature of this new technique.

The climatological mean distribution of temperature and salinity now seems to be fairly complete for the North Atlantic Ocean, especially in the surface layers. Fuglister's recent article in *Tellus* is most up-to-date. And a chart of the climatological mean distribution for the North Pacific is just on the verge of being finished. With the resources that we have had at hand it has been no mean task to get even this far. An analogous task for meteorologists would be the development of the climatological mean distribution of temperature and humidity over the northern hemisphere by using half a dozen automobiles and kites to which air sounding instruments were attached and by doing all of their work on dark moonless nights when they couldn't see what was happening in their medium.

It is inevitable now that the survey type of expedition is reaching the end of its usefulness to physical oceanography. What *do* oceanographers do when they have completed the climatological mean picture of oceans of the northern hemisphere! It is in this connection that it is instructive to attempt to draw from meteorological science some ideas about what we can hope to investigate and how we might go about it.

To begin with, we cannot propose an ocean-wide network of observation stations similar to the radiosonde stations available to meteorologists. We are not justified in doing so because hardly anybody

wants detailed forecasts of oceanographic conditions badly enough. Thus, it seems, oceanographers are immediately excluded from the types of analysis which are the very backbone of many aspects of meteorological science, such as the detailed aerological studies of the motion and development of large-scale disturbances that play so large a rôle in synoptic meteorology, the statistical studies of mean transport of properties across latitude circles, and so on.

Going back to our analogy again, let us assume that the meteorologists have completed their climatological mean distribution with their half dozen perambulating automobiles and kites; if they were then denied the use of a *wide* network of upper air stations, they would undoubtedly settle for as many permanent upper air stations as they could get. They would set up these stations in the best locations they could think of and would do everything possible to make observations of winds as well as of temperature and humidity distribution. From that time on they would probably use their automobiles primarily as a means of transportation to and from their few precious weather stations.

It seems to me that oceanographers should now give attention to the problem of setting up permanent oceanographic observatories, using moored buoys and midocean islands. The only available continuous observations for oceanographic purposes are (a) surface observations obtained by tide gauges and lightships along the shallow margins of the sea and (b) more recently a series of temperature measurements taken by bathythermograph to 900 feet at weather ships. Serial observations of the deep structure of the ocean have been made for a few days at a time at perhaps two dozen anchor stations. Anybody who has ever tried to analyze these anchor station data for anything such as internal tides will know how hopelessly inadequate these short series are. Would any meteorologists care to generalize on the diurnal surface pressure fluctuation by using absolutely nothing but two or three days' barograms from Charleston?

Last night I asked Dr. Palmén if he would suggest an intelligent question that I might ask him at this time. He replied, that, inasmuch as he had specifically avoided discussing deep-sea data, he thought I could not ask an intelligent question, and I think he was right. However, we did discuss the problem of making precise measurements of wind stress in the ocean by trying to measure the mass transport of water to the right of the wind by means of a buoy network or otherwise. It was Dr. Palmén's belief that there are too many complicating factors in the response of the ocean to a suddenly applied wind stress to permit valid measurements of wind stress in the deep ocean. In fact he believes that large shallow basins, such

as the Baltic Sea, where he has already obtained such definite results, are preferable. But these very complications need our attention. We have no direct knowledge about what a storm does to the velocity field at all depths. There are many complications involved in the response of a stratified ocean to the passage of a storm, many internal modes of motion; there is the possibility that the surface pressure field can induce water motions even at the bottom of the ocean. If it is hopeless for oceanographers to provide useful data on stress at the sea surface, then perhaps they might at least investigate the complicating factors.

Similarly, so far as the temperature field is concerned, successive bathythermograms at a fixed deep ocean station (weather ship data) show fluctuations in structure which are attributed to (a) advection, (b) diurnal heating, (c) wind mixing, (d) free internal waves, (e) forced changes in depth of density surfaces accompanying horizontal convergences due to atmospheric disturbances, (f) internal tides, (g) macroturbulence, etc. It would be a wonderful thing if a network of telemetering buoys could be used some day in an attempt to sort out some of these wonders of the deep.

So far as the development of large-scale meanders and eddies in ocean currents is concerned we do have some synoptic charts of such a development. In fact we have two successive charts. Would any meteorologists care to make generalizations about the development of cyclones and anticyclones over the United States from two successive surface temperature charts, and nothing else?

I do hope you will not think I am trying to paint a black picture. What I am trying to do is to convince meteorologists that we oceanographers have nothing that compares to a world-wide network of radiosonde stations and that what we can hope to achieve in the immediate future in our understanding of the ocean will be determined to some extent by what one can hope to learn from a very limited number of permanent observation points. And I am trying to convince oceanographers that their need for permanent observation points has passed the stage of idle discussion and has become an essential need.

Now, it is important for us to realize that all of the large-scale problems that meteorologists are working on do not depend upon their worldwide network of radiosonde stations. For example, Bunker's studies of the turbulent exchange of heat and water vapor which occurs as an air mass moves out over the first few hundred miles of sea depends primarily upon soundings made from a single airplane. Studies by aircraft of tropical storms which often occur in remote oceanic areas outside the network are another example. Certainly investigations of the high atmosphere by means of rockets

are of a kind not dissimilar to the type of observational program which is available to oceanographers. We can certainly learn much by tracking clusters of drifting buoys (by wireless-direction-finding or by SOFAR) just as meteorologists are beginning to use constant level balloons to map air trajectories. In a recent issue of the *Journal of Meteorology* there was an account of tracking groups of balloons for thousands of miles and recovering them on the same patch of ground—direct evidence of the small amount of lateral mixing present. We need to make similar studies in ocean areas where we imagine large-scale lateral mixing occurs, just to see if it is really there after all. Were it possible to design cheap neutrally buoyant floats which could signal their position by means of a SOFAR network, then we could start immediately to study the deep water circulation of the sea.

In regard to Dr. Charney's interesting remarks on quasigeostrophic flow, it is worthwhile to repeat the unhappy fact that we have little observational information that can be of direct use in applying such ideas. We have no data for studying time sequences of events over intervals of many pendulum days. Rossby's early papers on the mutual adjustment of pressure and velocity fields in an ocean current, in some respects similar to Dr. Charney's example, were also developed as a sequence of processes which slowly vary with time and which follow an initially concentrated unbalanced current; but he applied them toward an explanation of the *spatial* sequence of downstream features of the Gulf Stream which appear after it has left the Florida Straits. According to our recent experience, however, there is little variation in the Gulf Stream velocity or density structure for many hundreds of miles after it passes Cape Hatteras, and its final dissolution is catastrophic or abrupt—a manner quite the opposite of a gradual, slowly varying process. I recognize the fact that Dr. Charney's ideas are a useful tool for large-scale planetary phenomena. I think it would be helpful to oceanographers in visualizing the meaning of such ideas oceanographically if data on some actual states of affairs could be obtained; that is, if some real ocean phenomena to which these ideas might be applicable could be made available. This might lead to types of observational programs which would supply the data necessary for their application.

Comparison of meander-waves in the Gulf Stream, as well as of waves in atmospheric currents, is hampered by lack of information on the structure of meander-waves. The basic current, the Gulf Stream itself, is a steady phenomenon—perhaps more so than most atmospheric currents, and for that reason it may actually lend itself more easily to dynamical explanation.

As an example of the possible dynamical simplicity of the Gulf Stream, I should like to exhibit the results of a vorticity analysis of Worthington's 1950 hydrographic traverse perpendicular to the Stream, at about 68° 20′ W, across what appears to be a very straight portion of the Stream.

The potential vorticity of the water between any two isotherms is defined as

$$\frac{f+\zeta}{h} \quad , \tag{1}$$

where f is the Coriolis parameter, ζ the relative vorticity, and h the vertical distance separating the two isotherms. In the "free stream region" of the Gulf Stream, only the axial component of velocity v is important, and for this reason we are justified in replacing the relative vorticity by the term $\delta v/\delta x$. For convenience we direct the y-axis along the Stream and the x-axis toward the Sargasso Sea. Since there is so little evidence of any mixing or frictional processes in the region of the free stream, it is reasonable to suppose that the potential vorticity of the water between any two isotherms is uniform horizontally even though the thickness between the two isotherms changes as a function of position across the Stream. Moreover, in the Sargasso Sea itself we note that the relative vorticity nearly vanishes so that we are justified in expressing the conservation of potential vorticity between any two isothermal surfaces in the following form:

$$\frac{f+\partial v/\partial x}{h} = \frac{f}{h_0} \quad , \tag{2}$$

where h_0 is the vertical distance between isothermal surfaces in the resting water of the Sargasso Sea. Thus the axial velocity of the water between any two isothermal surfaces should be determined by the following relation:

$$v = \int_{\infty}^{x} f\left(\frac{h}{h_0} - 1\right) dx \quad . \tag{3}$$

If velocity determined in this way is similar to the geostrophic velocity as determined from dynamic computations, then we may feel that the hypothesis concerning the conservation of potential vorticity is supported by facts. Fig. 2 shows the results of such a comparison; the upper part shows the vertical thickness of the water layers between the 17° and 19° C isotherms, across the Gulf Stream; the lower part shows the comparison between the velocity as given by Eq. (3)

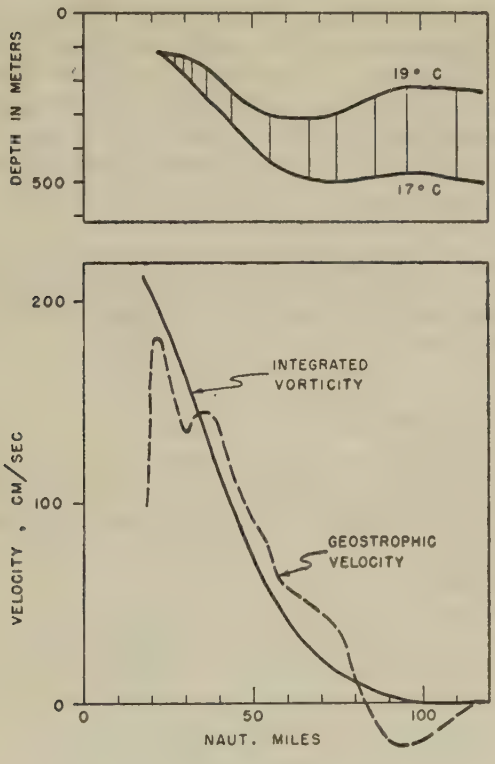

Figure 2.

(solid line) and the geostrophic velocity. The two curves agree surprisingly well. Similar constructions for other pairs of isotherms down to the 8° isotherm give similar results. We take this as evidence that potential vorticity is actually conserved in the Gulf Stream.

The conservation of potential vorticity in the Stream is important because it determines its width and velocity. If we were to make the Stream wider or narrower in an arbitrary fashion, then the two velocity curves in Fig. 1 would not coincide.

The geostrophic law defines the total transport of the Stream, not its velocity profile. The first theoretical explanation of the width of the Stream was that given by Munk in his linearized theory of wind-driven ocean circulation. He found that he could control the width of the Stream by varying the value of the coefficient of lateral eddy viscosity. This is, of course, a rather unsatisfactory feature of the theory because almost nothing is known about the value of

this coefficient in the vicinity of the Gulf Stream. In fact, most recent evidence suggests that the coefficient must be much smaller than the value Munk used. By diminishing the coefficient, we would be led by Munk's theory to a Gulf Stream that is much *narrower* than that actually observed, were it not for the essentially nonlinear effect of conservation of potential vorticity which now appears to dominate the dynamics of the Stream and to give a unique velocity profile.

This analysis is possible only because of the close spacing of Worthington's traverse. One cannot help wondering whether meteorologists will discover the same thing about the atmospheric jet if they make a closely spaced cross-section of it sometime.

C. W. THORNTHWAITE:

At this stage of the proceedings, very little is expected of me. Accordingly, I will limit my remarks to four or five minutes. In that brief time I can only indicate by means of an example or two how climatology has been able to contribute to the solution of some important oceanographic problems.

Among other things, in climatology we are interested in the water exchange between the earth's surface and the atmosphere. We cannot tell whether a climate is moist or dry merely by measuring the rainfall. It is necessary to determine the relation between what falls as rain and what is lost through evaporation, or through evapotranspiration. If one determines the water needs of an area and relates those needs to the water supplied through rainfall, a bookkeeping system can be set up to determine whether the rainfall is greater or less than the need and whether there is in consequence a water surplus or a water deficiency.

These relations can be expressed graphically. In Fig. 3 the march of average precipitation and of potential evapotranspiration through the year are shown for Seabrook, New Jersey; Berkeley, California; Montgomery, Alabama; and Albuquerque, New Mexico. At Seabrook the potential evapotranspiration is negligibly small in winter, but in early spring it begins a rapid rise which reaches the high point of the year in July with more than 15 cm. It falls rapidly during the autumn months. The corresponding precipitation is far more uniformly distributed through the year, being close to 9 cm in nine of the twelve months. The rainiest months are July and August, when each receives slightly more than 11 cm; November, the driest month, has only 7 cm.

In this example rainfall and water need do not coincide. There is too much rain in winter and too little in summer. Thus, at the time of maximum rainfall in July and August, there is a water deficiency, whereas in November, when rainfall drops to the lowest value of the year, there is a water surplus. In early autumn, water need falls below precipitation. For a while the surplus rainfall replaces soil moisture that had been used up previously. From then on the surplus water raises ground-water levels and produces surface and subsurface runoff. In spring both transpiration and evaporation increase rapidly, so that water need soon surpasses precipitation. Thereafter, through early summer, the excess demands for water are satisfied from the soil moisture reserves. When these reserves are exhausted, evapotranspiration is limited to the current rainfall. This shows that if the vegetation is to have all the water it needs and if the crops are not to suffer from drought, additional water must be provided through irrigation.

From study of this graph we see that there are two important climatic elements that can be derived from comparison of precipitation with potential evapotranspiration. One is the water surplus, which occurs in winter in Seabrook and which amounts to about 40 cm, and the other is the water deficit, which occurs in summer and which amounts to about 10 cm. Through the course of the year there is a net water surplus amounting to 30 cm. Through this system of water bookkeeping it is possible also to determine the water that must be accounted for as soil moisture storage.

The three other illustrations in Fig. 3 show the relation between water supply and water need in other climates. Berkeley receives nearly all of its rainfall in winter and almost none in summer. Accordingly, the winter water surplus and the summer water deficit are both comparatively large; the surplus is 23 cm, the deficit 31 cm. Montgomery is similar to Seabrook. However, the water need is greater in every month and the average precipitation is less uniform through the year, ranging from 16 cm in March down to 6 cm in October. Montgomery, like Seabrook and Berkeley, has a water surplus in winter and a water deficit in summer. Albuquerque is an arid station where precipitation satisfies the water need only in the three winter months but produces no water surplus and where the water deficit otherwise is very large.

The method outlined above, whereby water need and water supply are compared, has a number of practical applications. Water surplus comprises accretion to ground water and surface runoff which is equivalent to stream flow. Thus the method enables one to use climatological records for the determination of values of stream flow

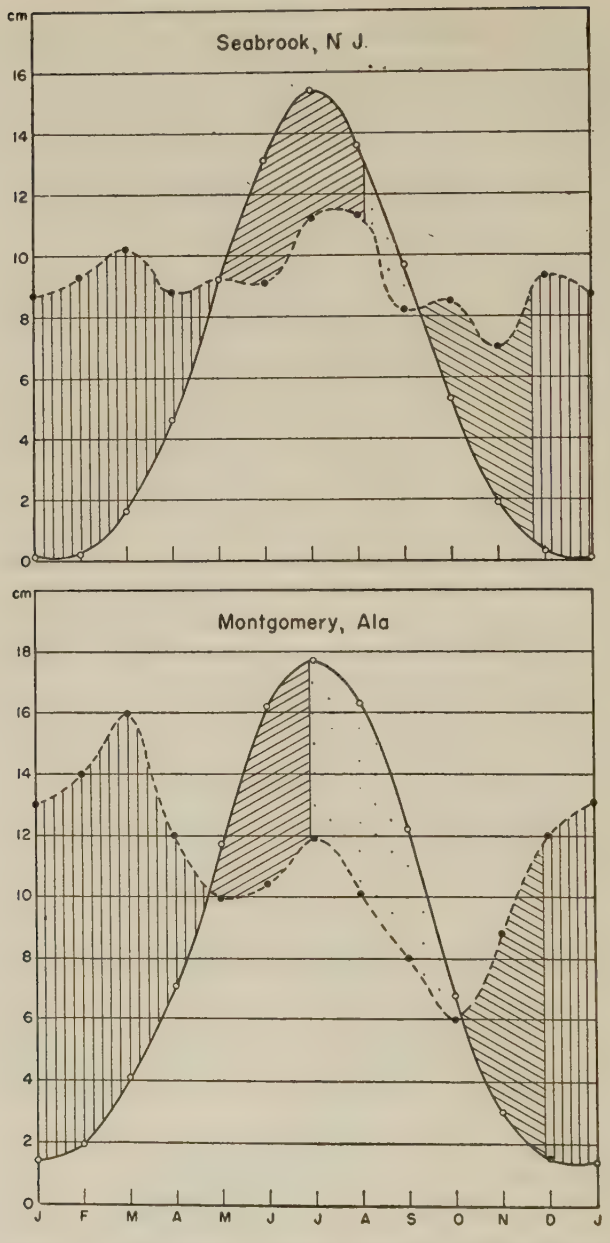

Figure 3. Water balance of selected stations in the United States.

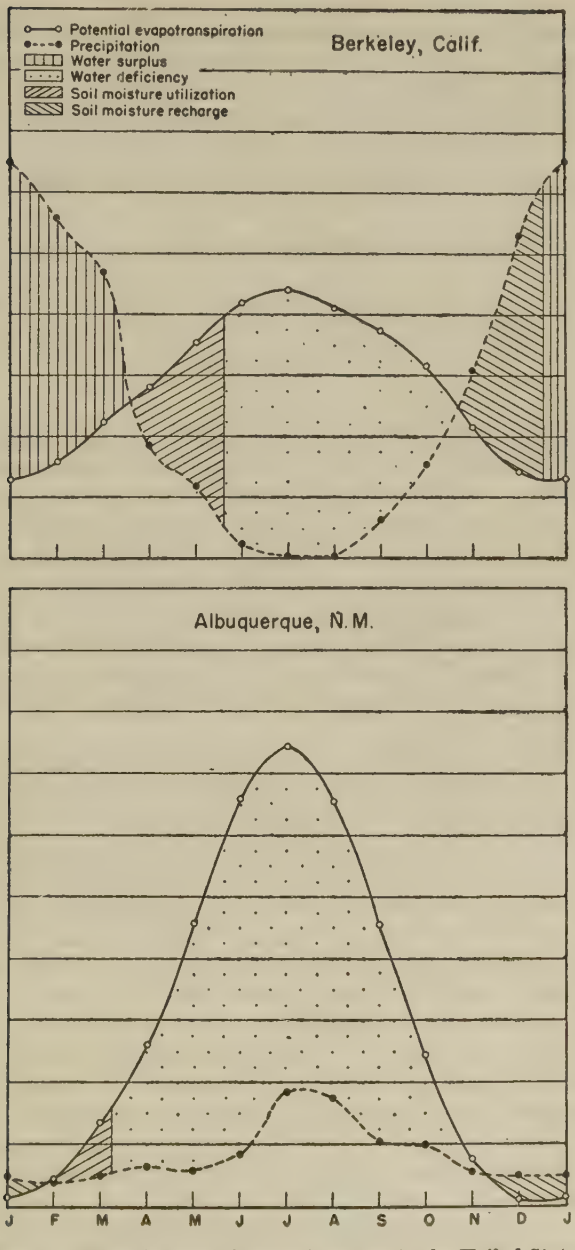

Figure 3. Water balance of selected stations in the United States.

in areas where no stream gage records exist. The method also keeps account of soil moisture storage and ground water accretion. It is thus possible to estimate from climatological data the amount of water temporarily stored on the land in any time of year.

The total water temporarily in storage at the end of each month at the four stations summarized in Fig 3 is given in Table I.

TABLE I. Total Temporary Water Storage at Selected Stations (in cm)

	J	*F*	*M*	*A*	*M*	*J*	*J*	*A*	*S*	*O*	*N*	*D*
Seabrook	16.1	17.6	*18.1*	16.2	13.1	7.5	2.6	0.4	*0.2*	3.3	8.3	13.7
Berkeley	15.1	*16.5*	15.7	11.0	4.8	0.7	0.4	0.2	0.1	*0*	1.9	9.7
Montgomery	17.4	19.7	*20.8*	17.9	12.1	4.4	1.0	0.5	0.2	*0.1*	5.9	13.2
Albuquerque	*1.5*	1.4	0	0	0	0	0	0	0	0	0	0.8

In Seabrook, for example, at the end of March 18.1 cm of water are temporarily trapped in the soil, in the ground water reservoir, and in the streams. At the end of September the water in temporary storage has fallen to 0.2 cm; the range between maximum and minimum values is 17.9 cm. In Berkeley, the difference between the February maximum and the October minimum is 16.5 cm. In Montgomery, the range between March and October is 20.7 cm. In Albuquerque, on the other hand, the precipitation is never great enough in any month to produce a water surplus and consequently no water except a meager amount of soil moisture is held temporarily in storage on the land.

These examples introduce an important oceanographic problem that has been a special concern of Walter Munk for some time. By studying the records of tide gages from all over the world he has found that the oceans contain less water in March than in October. He seems to have lost 5.10^{19} grams of water. There are two possible places to look for this missing water: the atmosphere and the land. By extending the bookkeeping methods used for individual stations (as illustrated in Fig. 3) over the whole land area of the earth it becomes possible to determine how much water is in temporary storage on the land and in transit over it.

In the northern hemisphere the greatest supply of water is locked up in the land in spring while the minimum level occurs in autumn. This is confirmed by the graphs of Seabrook, Berkeley, and Montgomery. Although there are local exceptions, it is generally true that a maximum of water is locked up in the land in the form of snow, soil moisture, ground water and stream detention storage in March and that a minimum is present in October, just as Dr. Munk would like to have it.

But in the southern hemisphere the seasons are reversed and so are the months of maximum and minimum water storage in the land. However, the land area of the southern hemisphere is so much smaller than that of the northern that conditions in the northern hemisphere prevail. It would seem that the missing water has been found, but completion of the inventory, country by country, remains to be done to see if we get the answer that Dr. Munk requires.

Another problem is one that was presented by Dr. Alfred Redfield. In a letter which he wrote to me in August 1953, he said, "Oceanographers have recently developed an active interest in the circulation of estuaries, and in this connection data on the accessions of fresh water from river flow and seepage are essential. In many cases river flow is not adequately gauged and seepage may also become an important factor. For this reason, we would like to estimate fresh water accession from rainfall corrected for evapotranspiration."

Then he went on to give a case in point; a large coastal lake is linked to the ocean from which it receives salt water sometimes and to which it loses fresh water at other times. It is in a part of the world where no stream-gaging stations exist and where there is no possibility of getting them in time to aid in the solution of the problem.

By making use of data from the climatological stations in the catchment area tributary to this lake and by determining the potential evapotranspiration as well as the water surplus and other elements of the water cycle month by month, it has been possible to work out the water economy of the lake to show that in certain months it has an excess of water which flows into the ocean and in others a deficit which is made up by inflow from the ocean. This study is not yet complete; but the work done so far demonstrates that the methods of climatology are indeed of real assistance in the solution of oceanographic problems.